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Accounting Scholarship that Advances Professional Knowledge and Practice

Robert S. Kaplan
Harvard University

ABSTRACT: Recent accounting scholarship has used statistical analyses on asset prices, financial reports and disclosures, laboratory experiments, and surveys of practice. The research has studied the interface among accounting information, capital markets, standard-setters, and financial analysts and how managers make accounting choices. But as accounting scholars have focused on understanding how markets and users process accounting data, they have distanced themselves from the accounting process itself. Accounting scholarship has failed to address important measurement and valuation issues that have arisen in the past 40 years of practice. This gap is illustrated with missed opportunities in risk measurement and management and the estimation of the fair value of complex financial securities. This commentary encourages accounting scholars to devote more resources to obtaining a fundamental understanding of contemporary and future practice and how analytic tools and contemporary advances in accounting and related disciplines can be deployed to improve the professional practice of accounting.

Keywords: *accounting research; professional practice; accounting education; field studies; risk measurement; pension risk; fair value measurement; options pricing model.*

I. INTRODUCTION

My talk is a response to a request from a 28-year-old, newly minted doctoral graduate soliciting advice about how to select research topics and teaching assignments for her forthcoming career as an accounting academic.

You are embarking on an exciting five-decade faculty career as a business school professor. You have acquired knowledge of contemporary accounting research issues and excellent disciplinary training in mathematics, economics, finance, statistics, and the behavioral sciences. But you should understand that you are joining the faculty of a professional school, not a graduate school or academic department. Your profession could be viewed as "accounting" or somewhat more

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I am indebted to two HBS colleagues: Robert Merton, who inspired me with the opportunities to advance accounting scholarship and practice by building upon his pioneering work in financial economics; and Rakesh Khurana, for discussions about the role for scholarship in professional schools. I also benefited from discussions with Scott Richard, Wharton School, about the feasibility and desirability of fair value measurements for financial instruments.

Editor's note: This commentary, based on a lecture at the 2010 American Accounting Association Annual Meeting in San Francisco, CA, was invited by Senior Editor Steven Kachelmeier, consistent with the AAA Executive Committee's goal to promote broad dissemination of the AAA Presidential Scholar Lecture.

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broadly as "management" (Khurana 2007). Among all the management disciplines, accounting is the closest to a professional discipline—many of the students we educate get certified and must maintain professional standing through continuing education programs. These accounting and financial professionals are our clients.

As a scholar in a professional school, you will educate current and future professionals on the field's common body of knowledge. You will learn how to dispense the existing common body of knowledge in ways that your students can put into action during their professional careers. All accounting and management professors have this responsibility. But also important, though less widely known, accepted, and advocated, academic scholars at a professional school should contribute to *advancing* the profession's body of knowledge, especially when innovation is high and major changes are occurring in the practice environment of the profession.

For building and sustaining a successful scholarly career in a professional school, you should be continually thinking about three fundamental questions:

1. What are the big issues faced by our practice community?
2. What are the comparative advantages that accounting scholars can bring to address these fundamental accounting and management issues? My partial list for areas in which accounting scholars should have a comparative advantage, relative to scholars in other managerial disciplines, includes measurement, reporting and disclosure, auditing, financial analysis, and management control.
3. How does our research advance knowledge in these core areas of our discipline?

II. ACCOUNTING RESEARCH: 1968–2010

During the past 40+ years, research by management and accounting scholars has produced many important insights and advanced our understanding of the environment in which accounting functions. Much of this scholarship has focused on how information and markets interact, including the information content of accounting numbers, the role of accounting accruals versus cash flows, voluntary and involuntary managerial disclosures, efficiency and anomalies in markets, and the impact of accounting choices made by managers. Some scholarship has given us insights in how financial analysts do their work and their influence on markets. I have been particularly impressed by the development of the accounting-based valuation model (Ohlson 1995; Feltham and Ohlson 1995). This is a particularly good, although somewhat rare, example of where academic accounting scholarship has created an important innovation that advanced professional practice. Academics have contributed to standard-setting practice by testing the reactions to proposed and implemented standards and communicating the implications of capital-market-based research to standard-setters. Accounting scholarship has also informed and advanced the professional work of auditors and tax professionals (AAA Research Impact Task Force 2009).

Most of these research contributions have come from statistical studies of phenomena, using archival data from financial statements, proxy statements, market prices of equity, debt, and occasionally other financial instruments, credit ratings, loan documents, bankruptcies, conference calls, and analyst forecasts. Accounting scholars have also performed statistical analysis on data collected from carefully designed laboratory experiments and simulations and from surveys of practitioners. One characteristic of all this research is the existence of multiple tables within the article populated with estimated coefficients, at least a few of which have between one and three asterisks near them, attesting to their degree of statistical, although not necessarily economic, significance (McCloskey 1998). Scholars have also studied, both analytically and empirically, the properties of management contracts and incentives.

Table 1, extracted from the annual reports of *The Accounting Review* over the past two years (Kachelmeier 2009, 2010), summarizes the research methods used by accounting scholars in the

TABLE 1

Submissions and Acceptance to *The Accounting Review* (2008–2010)

Research Method	# Submitted	# Accepted	% Submitted	% Accepted
Empirical-Archival	933	93	75%	65%
Experimental	175	24	14%	17%
Analytic	96	18	8%	13%
Survey	30	6	2%	4%
Field and Case Study	15	2	1%	1%
Total	1249	143	100%	100%

Source: Kachelmeier (2009, 2010).

papers submitted and accepted during the two-year period 2008–2010 to the American Accounting Association's leading research journal, *The Accounting Review*. The table shows the predominance of statistical analysis on archival, mostly financial, data, and also the extensive use of laboratory experiments, surveys, and analytic models. For future reference, note the low market share (1 percent) of field and case studies.

Overall, I think this is a good record, in fact a very good record, of accomplishment. Compared to 45 years ago, we have had a great expansion of research performed and published by a broader academic accounting community, and some of this research has helped us understand, much better than in 1968, the role of accounting reporting and disclosure in markets and contracts, and how people process and use accounting information. But we are likely encountering diminishing returns in several of our research areas. The increment in our knowledge from the 371st paper on accruals versus cash flows, earnings management, or voluntary disclosure is undoubtedly much lower than the contribution from the first five to ten papers that introduced each topic into the academic accounting literature. Accounting scholars, like many of their academic colleagues, exhibit strong herding effects; they follow where others have already gone rather than forging a new path by studying a new issue in an innovative way.

III. THE GAP BETWEEN ACCOUNTING SCHOLARSHIP AND PRACTICE

If academics in professional school limit their research agenda to issues that can be adequately addressed by a narrow set of generally accepted research methods, then they must wait for phenomena to happen to generate sufficient archival data for them to analyze statistically. Thus, much of accounting (and management) research for the past 40 years has been reactive. It has studied, evaluated, and explained existing practice, but has not contributed to advancing that practice (the accounting-based-valuation model, and improvements in auditing and tax being notable exceptions). This situation would be adequate if little change or innovation was occurring in the profession. But over the past 50 years, some major discontinuities have occurred and the implications of these changes have yet to make their way into the common body of knowledge being studied by academic scholars and taught to future professionals. Among these changes are huge increases in the volume, velocity, volatility, and complexity of transactions, the globalization of markets leading to trading of assets in different time zones and currencies and to issues in reporting and managing across unclear organizational boundaries, and the introduction of new and complex securities and derivative contracts that shift risks among companies and market participants. Society has increased its expectations about corporate governance and oversight of executive compensation. A major management technology innovation has occurred for pricing risk (Black and Scholes 1973; Merton 1973), which has been overlooked and underappreciated by accounting scholars for the past four decades (much more on this later).

By responding slowly if at all to major new challenges and opportunities in the environment in which accounting is practiced, accounting scholars have become less familiar with emerging professional challenges and opportunities. Few have helped to craft new solutions and approaches for people practicing in the profession. For example, we failed to see in a timely fashion the reporting, valuation, and disclosure implications from the new types of mortgage lending and the massive securitizations of these loans.

Interestingly, accounting scholars are not the only academics who have become disconnected from practice. Paul Krugman (1994, 43–44) describes a similar situation in development economics, which he called the "hollowing of Africa" effect:

> The paper, "The Evolution of European Ignorance about Africa," describes how European maps of the African continent evolved from the 15th to the 19th centuries ... The coastline of Africa was first explored, then plotted with growing accuracy, and by the 18th century that coastline was shown in a manner essentially indistinguishable from that of modern maps ... On the other hand, the interior emptied out. The weird mythical creatures were gone, but so were the real cities and rivers. In a way, Europeans had become more ignorant about Africa than they had been before ... improvement in the art of mapmaking raised the standard for what was considered valid data. Second-hand reports of the form "six days south of the end of the desert you encounter a vast river flowing from east to west" were no longer something you would use to draw your map. Only features of the landscape that had been visited by reliable informants equipped with sextants and compasses now qualified.

Today's accounting academics, after 40 years of using rigorous social science research methods, know much more about the coastline of accounting, its interface between accounting reports and capital markets, analysts, auditors, regulators, boards, and the media. But they know substantively less than academics of 40 years ago about the "interior" of leading-edge professional accounting and finance practice. We have experienced a "hollowing of professional practice" among accounting scholars that you and a new generation of accounting faculty could productively fill.

More than 30 years ago, Roethlisberger, a social scientist and management scholar who was a coauthor on the Hawthorne effect experiments, described the research preferences of academic social scientists to work on topics that can be explored using statistical and mathematical methods (Roethlisberger 1977, 390). He illustrated his points using the Knowledge Enterprise diagram in Figure 1.[1] Roethlisberger observed that the clinical approach of acquiring knowledge by describing and classifying the activities of skilled practitioners is frequently bypassed by researchers because clinical research is considered less elegant than deductive research.[2]

Donald Schön (1992, 54) also expressed concern that systematic study and description of professional practice holds a less exalted view in academia than deductive theory and hypothesis testing:

> In the varied typography of professional practice, there is a high, hard ground which overlooks a swamp. On the high ground, manageable problems [N.B. think of simple principal-agent models] lend themselves to solution through the use of research-based theory and technique. In the swampy lowlands, problems are messy and confusing and incapable of technical solution ... [But] the

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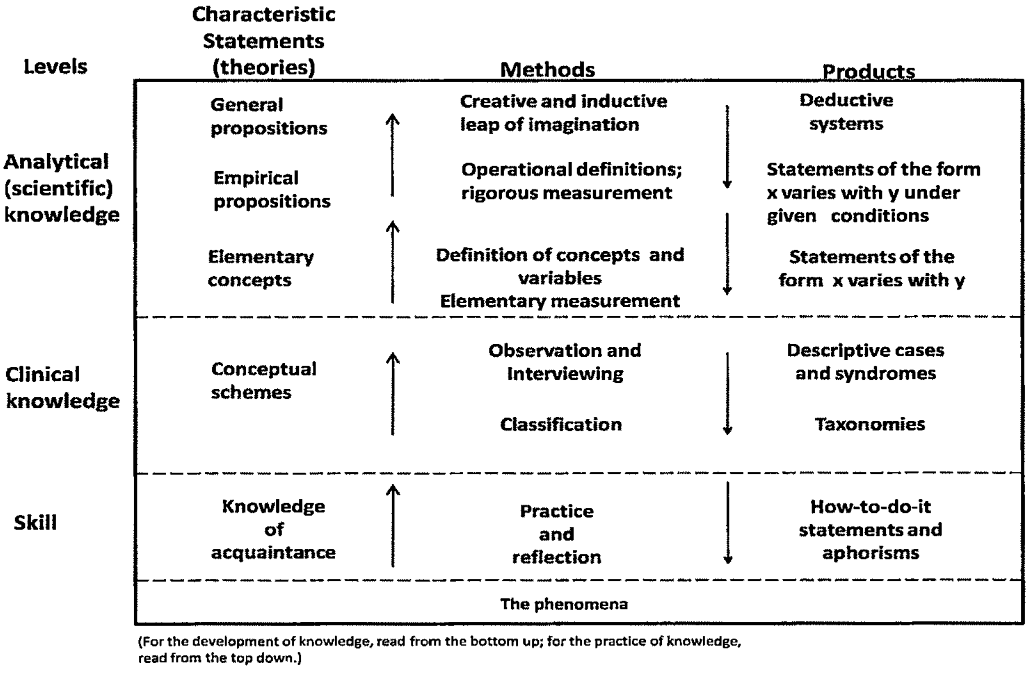
[1] Summaries of the activities at the various stages of the knowledge tree—description, classification, measurement, discovering relationships, theory building, and theory testing—can be found in Kaplan (1986).

[2] On academic scholars' desire for elegance, I add a comment by Einstein (1920, 5 [Preface]), "leave elegance to the tailor," which he attributed to the 19th century physicist, Ludwig Boltzmann.

FIGURE 1
Knowledge Enterprise Diagram



Levels	Characteristic Statements (theories)		Methods		Products
Analytical (scientific) knowledge	General propositions	↑	Creative and inductive leap of imagination	↓	Deductive systems
	Empirical propositions	↑	Operational definitions; rigorous measurement	↓	Statements of the form x varies with y under given conditions
	Elementary concepts	↑	Definition of concepts and variables Elementary measurement	↓	Statements of the form x varies with y
Clinical knowledge	Conceptual schemes	↑	Observation and Interviewing Classification	↓	Descriptive cases and syndromes Taxonomies
Skill	Knowledge of acquaintance	↑	Practice and reflection	↓	How-to-do-it statements and aphorisms
			The phenomena		

(For the development of knowledge, read from the bottom up; for the practice of knowledge, read from the top down.)

Source: Roethlisberger (1977, 393).

> problems of the high ground tend to be relatively unimportant to individuals or to society at large, however great their technical interest may be, while in the swamp lie the problems of greatest human concern ... Some researchers ... pursue an agenda driven by evolving questions of modeling theory and techniques, increasingly divergent from the contexts of actual practice.

I am less pessimistic than Schön about whether rigorous research can inform professional practice (witness the important practical significance of the Ohlson accounting-based valuation model and the Black-Merton-Scholes options pricing model), but I concur with the general point that academic scholars spend too much time at the top of Roethlisberger's knowledge tree and too little time performing systematic observation, description, and classification, which are at the foundation of knowledge creation. Henderson (1970, 67–68) echoes the benefits from a more balanced approach based on the experience of medical professionals:

> both theory and practice are necessary conditions of understanding, and the method of Hippocrates is the only method that has ever succeeded widely and generally. The first element of that method is hard, persistent, intelligent, responsible, unremitting labor in the sick room, not in the library ... The second element of that method is accurate observation of things and events, selection, guided by judgment born of familiarity and experience, of the salient and the recurrent phenomena, and their classification and methodical exploitation. The third element of that method is the judicious

> construction of a theory … and the use thereof … [T]he physician must have, first, intimate, habitual, intuitive familiarity with things, secondly, systematic knowledge of things, and thirdly an effective way of thinking about things.

More recently, other observers of business school research have expressed concerns about the gap that has opened up in the past four decades between academic scholarship and professional practice. Examples include:

> Historical role of business schools and their faculty is as evaluators of, but not creators or originators of, business practice. (Pfeffer 2007, 1335)

> Our journals are replete with an examination of issues that no manager would or should ever care about, while concerns that are important to practitioners are being ignored. (Miller et al. 2009, 273)

In summary, while much has been accomplished during the past four decades through the application of rigorous social science research methods to accounting issues, much has also been overlooked. As I will illustrate later in these remarks, we have missed big opportunities to both learn from innovative practice and to apply innovations from other disciplines to important accounting issues. By focusing on these opportunities, you will have the biggest potential for a highly successful and rewarding career.

Integrating Practice and Theory: The Experience of Other Professional Schools

Other professional schools, particularly medicine, do not disconnect scholarly activity from practice. Many scholars in medical and public health schools do perform large-scale statistical studies similar to those done by accounting scholars. They estimate reduced-form statistical models on cross-sectional and longitudinal data sets to discover correlations between behavior, nutrition, and health or sickness. Consider, for example, statistical research on the effects of smoking or obesity on health, and of the correlations between automobile accidents and drivers who have consumed significant quantities of alcoholic beverages. Such large-scale statistical studies are at the heart of the discipline of epidemiology.

Some scholars in public health schools also intervene in practice by conducting large-scale field experiments on real people in their natural habitats to assess the efficacy of new health and safety practices, such as the use of designated drivers to reduce alcohol-influenced accidents. Few academic accounting scholars, in contrast, conduct field experiments on real professionals working in their actual jobs (Hunton and Gold [2010] is an exception). The large-scale statistical studies and field experiments about health and sickness are invaluable, but, unlike in accounting scholarship, they represent only one component in the research repertoire of faculty employed in professional schools of medicine and health sciences.

Many faculty in medical schools (and also in schools of engineering and science) continually innovate. They develop new treatments, new surgeries, new drugs, new instruments, and new radiological procedures. Consider, for example, the angiogenesis innovation, now commercially represented by Genentech's Avastin drug, done by Professor Judah Folkman at his laboratories in Boston Children's Hospital (West et al. 2005). Consider also the dozens of commercial innovations and new companies that flowed from the laboratories of Robert Langer at MIT (Bowen et al. 2005) and George Whiteside at Harvard University (Bowen and Gino 2006). These academic scientists were intimately aware of gaps in practice that they could address and solve by applying contemporary engineering and science. They produced innovations that delivered better solutions in actual clinical practices. Beyond contributing through innovation, medical school faculty often become practice thought-leaders in their field of expertise. If you suffer from a serious, complex illness or injury, you will likely be referred to a physician with an appointment at a leading academic medical school. How often, other than for expert testimony, do leading accounting professors get asked for advice on difficult measurement and valuation issues arising in practice?

One study (Zucker and Darby 1996) found that life-science academics who partner with industry have higher academic productivity than scientists who work only in their laboratories in medical schools and universities. Those engaged in practice innovations work on more important problems and get more rapid feedback on where their ideas work or do not work.

These examples illustrate that some of the best academic faculty in schools of medicine, engineering, and science, attempt to improve practice, enabling their professionals to be more effective and valuable to society.

Implications for Accounting Scholarship

To my letter writer, just embarking on a career as an academic accounting professor, I hope you can contribute by attempting to become the accounting equivalent of an innovative, world-class accounting surgeon, inventor, and thought-leader; someone capable of advancing professional practice, not just evaluating it. I do not want you to become a "JAE" (Just Another Epidemiologist). My vision for the potential in your 40+ year academic career at a professional school is to develop the knowledge, skills, and capabilities to be at the *leading edge* of practice. You, as an academic, can be more innovative than a consultant or a skilled practitioner. Unlike them, you can draw upon fundamental advances in your own and related disciplines and can integrate theory and generalizable conceptual frameworks with skilled practice. You can become the accounting practice leader, the "go-to" person, to whom others make referrals for answering a difficult accounting or measurement question arising in practice.

But enough preaching! My teaching is most effective when I illustrate ideas with actual cases, so let us explore several opportunities for academic scholarship that have the potential to make important and innovative contributions to professional practice.

IV. RISK MEASUREMENT AND MANAGEMENT

I (and probably many others) became aware of the gaps in risk management practice in 2007 when important institutions began to fail unexpectedly. As this trend continued through 2008, I felt a lot like Yogi Berra when he said, "It's déjà vu all over again." The 2007/08 risk management failures took me back to the early 1980s (Kaplan 1983; Johnson and Kaplan 1986), when we learned, to our surprise, that cost accounting and performance measurement, highly important accounting functions, were being done poorly by many enterprises.

Risk management is a great issue for accounting academics. It builds upon our fundamental comparative advantages as an academic discipline: expertise in measurement, valuation, reporting, disclosure, management control, auditing, and governance. The topic contains issues relevant for financial reporting, management control, and auditing, three of our most important subject areas in the American Accounting Association.

Among the risk management issues I would like to know more about, consider the following questions:

1. How can we measure or quantify risk? Measurement is about the past; even so-called leading indicators are measuring events that have already occurred. How can we quantify risk or develop risk indicators for an event that has not yet occurred and, we hope, may never occur? Quantifying risk exposure is a challenging measurement issue.
2. Will a single risk management system work for all types of risks, or do different types of risks require different types of risk management systems?
3. The optimal level of risk is not zero. How can companies introduce risk management without sacrificing their innovation and risk-taking activities? What is the appropriate balance between innovation and risk management, and how can this balance be maintained?

4. Much work is currently being done by regulators and standard-setters (COSO, Basel, rating agencies, stock exchanges, etc.) to promulgate rules and standards on companies' risk management practices:
    a. Is the practice of risk management sufficiently stable, mature, and understood that now is a good time to develop risk standards and regulations? Or is it better for companies to innovate and experiment with different risk management approaches before regulators standardize and codify practices?
    b. How can academic scholars participate in and document such innovation and experimentation?
    c. Does and should the optimal form of risk management vary by type of company, strategy, degree of innovation? How?
5. What is the relationship between a professional risk management officer and line management?
6. What is the relationship between risk management and internal audit?
7. Other than perhaps in financial institutions, what do people mean when they talk about "risk appetite"? When the Board is told it must determine the "risk appetite" for the company, how can risk appetite be quantified and made operational?
8. Can scholarship improve the reporting and disclosure of risk in company documents?

On this last question, I looked at a recent risk disclosure in a company's 10-K:

> In the event of changes in market conditions, such as interest or foreign exchange rates, equity, fixed income, commodity or *real estate valuations*, liquidity, availability of credit or volatility, our business could be adversely affected in many ways ... Further declines in real estate values in the U.S. and continuing credit and liquidity concerns could further reduce our level of mortgage loan originations and increase our mortgage inventory while adversely affecting its value. (emphasis added)

Does this sound like the risk exposure of a huge financial institution that would file for bankruptcy less than two months after this 10-K submission? Yet this was the "risk disclosure" in the 2008 second quarter filing of Lehman Brothers, a financial institution born in the South in the 1850s. Lehman survived the U.S. Civil War, World War I, the Great Depression of the 1930s, and World War II. It built its capital during the great post-World War II global expansion, and somehow failed after a 5 percent decline in U.S. real estate prices. After 40+ years of academic research on capital markets and financial economics, is Lehman's 10-K disclosure the best we can offer to quantify and disclose a company's risk exposure? I hope not.

At present, we do not know enough to address the risk management questions with research methods at the top of the "knowledge enterprise." If we rely on traditional social science research methods, we will provide sketchy answers and only with long lags. Risk management in organizations is highly complex and context-specific. It involves many moving and interconnected components including leadership, organizational structure, incentives, finance, management control, and ... risk and uncertainty. We are going to learn little by sitting in our offices and analyzing data that others produce. Similar to how we went about developing new approaches to the cost and performance measurement issues in the 1980s, we should start by using research methods at the base of the knowledge enterprise tree: identify, observe and describe the practice leaders, document and analyze the practice innovations, develop the associations of where risk management seems to be working well and where it has failed, and start to formulate the general principles underlying effective risk management practice.

While this research agenda will take years of work, I can describe several accounting topics in risk measurement that could be immediately addressed by creatively applying existing knowledge.

Measuring Pension Plan Risk

Pension accounting has been addressed by standard-setters for decades, yet little has been done on measuring and reporting the risk of pension plans. Measuring the risk of pension plans is important not only for companies with defined benefit pension plans, but also for states, municipalities, and even countries. Consider an example, adapted from Merton (1995, 473), of two companies, each with $100 million in net present value of pension liabilities (incidentally, the calculation of pension liabilities is a measurement that accountants have already out-sourced to actuaries):

> Company A holds $90 million in default-free government bonds whose durations are matched perfectly with the liabilities in its pension plan. Company B holds $25 million in such duration-matched government bonds plus $75 million in a diversified portfolio of common stocks. Which pension plan is riskier?

Today's accounting standards would report that A's plan is riskier because B's plan is "fully funded" while A's is underfunded by $10 million. But Company B holds the equivalent of a giant derivative. It could have held $100 million in duration-matched risk-free assets, but it made the equivalent of a $75 million total return on equity-for-debt swap. Now, we all know the equity risk premium is about 800 basis points higher return per year than the return from risk-free bonds. Compounded over many years, a diversified portfolio of common stocks should return far more than the risk-free portfolio. So how risky could B's equity-for-debt swap be?

Suppose the trustees of B's plan conduct the following thought experiment. Even though stocks are expected to out-perform bonds over long periods of time, how much would it cost to purchase insurance to guarantee that the return on the equity portfolio, 20 years from now, is not lower than what could be achieved in a zero-risk government bond portfolio? While I have not independently calculated the following, an expert in this field has estimated to me that such insurance would cost $15 million or more. So it would cost plan B's trustees at least $15 million to protect its asset holding from generating returns below the risk-free rate. In fact, Company B's plan is significantly riskier than Company A's because of its equity-for-debt swap.

Today, however, such quantification of this risk exposure appears nowhere in the company's financial statements even though the technology for assessing the risk of Company B's plan has been around for four decades, ever since the introduction of the options pricing model. How do investors and creditors get information about the self-insurance risk that companies (and states and countries) take on when their pension plans contain risky assets intended to pay the commitments from their fixed pension liability structure?

An accountant attempting to calculate the risk of a pension plan's assets would need to decide which options model, among many alternatives, to use. Merton describes that an options pricing model is like an autopilot on an airplane. Both process a great deal of information in a coherent and consistent way—perhaps better than human intuition and experience. But pilots understand the model and assumptions being used by the autopilot mechanism and are prepared to override them based on judgment and experience. Which accounting scholars have acquired the experience and judgment to know when and which options pricing models are valid and under what circumstances to override the model? A model that is highly accurate for short periods of time may have high errors for long-term valuation, while one that is approximately valid for long periods may be quite inaccurate for short time intervals. Merton posed the following challenge to the accounting profession:

> accounting does a good job at valuation but ... it is totally inadequate to deal with risk allocation, which ... is one of the critical issues today. Accounting must change in a major way to address this in the future. (Merton 1995, 470)

His challenge, however, has gone unaddressed. The entire focus of the FASB and IASB, and almost all accounting academics, has been to debate the valuation of assets and liabilities but not their risk.

What would a risk balance sheet look like? I do not know; if I did, I would be writing about it already. Yet, this is exactly the problem of not having scholarship follow contemporary practice. The technology to develop a risk balance sheet has existed for decades. To get started learning about this issue, I would attempt to learn from the most skilled practitioners I could find, probably in hedge funds, investment banks, and sophisticated diversified corporations whose financial officers must assess and manage every single day their risk exposure from mismatched asset and liability positions. This is a great issue where you could establish a reputation as the world's "risk balance sheet practice innovator and thought-leader." If successful, your framework would be taught throughout this century to accounting and business students, and would also be applied widely in practice.

Studying the Role for Credit Default Swaps

A credit default swap (CDS) represents another practice innovation that has profound implications for accounting and auditing practice. Introduced by J.P. Morgan in the mid-1990s, a CDS, similar to a deep out-of-the-money put option, offers an insurance policy on the default of risky debt. An increase in a CDS rate implies an increased likelihood of future asset price declines or an increase in the volatility of future cash flows. Accounting academics have conducted much accounting research on assessing credit and bankruptcy risk, yet have paid little attention to this new security that provides a pure signal on default likelihood. The CDS signal avoids the complications from bond or credit-specific features—such as sinking funds, collateral, and covenants—that make inferring company risk from bond yields a complex calculation.

Governments and legislators continue to debate the role of rating agencies, and how to improve their performance. The U.S. government just passed a 2,000-page bill to increase regulation of financial markets hoping to avoid future systemic risk from the failure of one or more large financial institutions. Many of the details for its implementation have been left to various regulatory agencies. Who do you think would do a better job of assessing the default risk of a major financial institution? A government regulator, who is paid less than a new assistant professor of accounting, or the aggregation of beliefs among sophisticated professionals betting hundreds of millions of dollars, and more, on understanding and pricing the risk of corporate default?[3] (This is, I hope, a rhetorical question.)

Accounting professionals can play an important role to improve the pricing of CDSs by their reporting and disclosure of information about all the debt, commitments, and contingencies incurred by a company (or public sector entity such as a state and country). Among the interesting questions, about which we currently lack adequate knowledge on the quality and reliability of CDS pricing, are the following:

1. Are CDS prices a valid estimate of default likelihood? Do they predict financial distress better than our current models of bankruptcy and credit risk? These questions must be answered affirmatively before we could consider shifting corporate risk measurement from bond-rating agencies and regulators to CDS securities markets.
2. What information is impounded in a CDS price? What new information causes a CDS price to change?[4]

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[3] I am indebted to Professor Scott Richard, of the Wharton School, for this provocative suggestion.

[4] I am encouraged to see an article in *The Accounting Review* (Callen et al. 2009) begin to address this topic, but this first CDS paper in an accounting journal appeared five years after they started appearing in finance journals, and 15 years after the original innovation itself.

3. What kinds of risk models do sophisticated participants in CDS markets use?

For you to become the world's thought-leader on the pricing and information content of credit default swaps, I do not recommend using only reduced-form statistical models of the correlations between CDS prices and various accounting and financial ratios. CDS pricing is an emerging practice area, and I would attempt to learn, through observation and description, about the models and processes used by leading CDS-pricing practitioners. You could then move on to classify the various approaches you encountered and conduct association studies to assess the performance of these practice-based models against alternative default prediction models, or the human judgment of bond-rating personnel and government regulators.

V. FAIR VALUE MEASUREMENT

A third opportunity for innovative and important faculty research and scholarship is the measurement of fair values. Unlike research on pension plan risk and CDS pricing, many accounting scholars are already doing correlational studies between fair values and security prices, motivated to a considerable degree by the FASB and IASB mandates for increased fair value measurement and disclosure. This research reinforces the benefits from epidemiological scholarship on contemporary practice issues. Yet all this scholarship takes the "fair values" as given and does not explore how fair values actually get estimated. The accounting profession is in danger of potentially outsourcing this critical measurement, and its validation, to others. Do we have, today, academic experts on how to do "fair value" analysis for complex assets? Who is our leading academic "fair value" thought-leader, the go-to person when a company or audit firm has a difficult question on a complex fair value calculation with a lot at stake?

Let us explore the state of the art today on determining fair values for financial instruments, which are, by far, the simplest applications for fair value measurements. The traditional accounting approach of using contemporary market prices works well for assets that trade continually in thick markets. For assets that are not actively traded, banks advocate and accounting educators teach the discounted cash flow approach, using the interest rate at the time the financial asset was issued. While current accounting standards require that impairments in these assets get recognized, most banks argue against recognizing impairments as long as debtors continue to make payments. This leads to nontraded or thinly traded financial assets being carried at historical cost (or terminal value). Wachovia, and other banks, resisted fair value reporting of their financial assets by classifying them into their "held-to-maturity" portfolios, a classification that defies economic substance except in a highly restricted case. Wachovia in July 2008, reported $75 billion in shareholders' equity, even after taking "modest" impairments of more than $10 billion during the previous 12 months in its more than $300 billion loan portfolio (valued at historical cost). Yet less than three months later, the bank had failed, and its acquirer, Wells Fargo, wrote down Wachovia's asset loan position by an additional $74 billion.

This incident, and many others at the time, reveals a major shortcoming in the contemporary financial reporting framework. The deterministic discounted cash flow model is not adequate for estimating the fair values of risky financial assets. And, sadly, the ability to estimate fair values of thinly traded financial assets has existed for decades.

Valuing a Risky Financial Debt Instrument

Again, I am in debt to my finance colleague Robert Merton, who proposed a simple (some would call it "elegant") model, based on the following equation, to calculate the value of risky debt (Merton 1974):

Risk-free debt = Risky debt + insurance policy on the value of the underlying asset.

This equation states that a holder of risky debt can transform the asset into risk-free debt by purchasing an insurance policy that pays off should the debt default (the insurance could be a default-free CDS, helping to explain the popularity of this financial instrument). After rearranging terms in the above equation, we obtain:

Risky debt = Risk-free debt

– insurance policy that the debtor ' s assets exceed the loan amount.

In other words, a bank lending to a consumer, commercial customer, or homeowner holds the equivalent of a risk-free debt position less a short position on an insurance policy that the debtor defaults. The following example makes this insight clearer:

> Consider a bank that makes a loan, L, which is secured by the assets of a company or homeowner currently having a value, V. The debtor now owns a put option for the face value of the loan because at the loan's maturity:

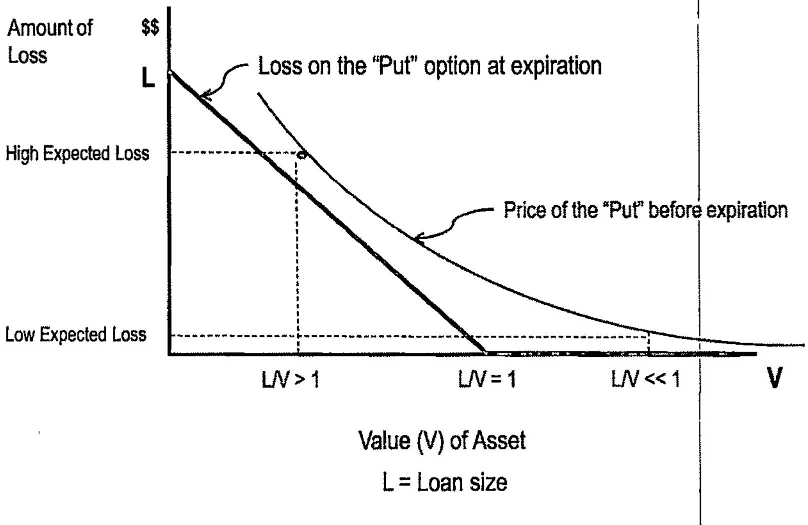
If V > L, the company (homeowner) repays the loan and banker gets back L.

If V < L, the company (homeowner) defaults and the bank loses L – V. Figure 2 shows the graphical representation of the bank's loss. The downward sloping line is the loss when the terminal value is less than the loan value. The horizontal portion shows that the loss will be zero if the asset value at loan maturity exceeds the loan value.

The hyperbola above this line in Figure 2 shows the value of the bank's short position in the put option that is embedded in this loan, for a time period before the loan's maturity. When V is much greater than L, the put option is deeply out of the money and there is essentially no decline in the fair value of the loan. But as the asset value approaches the loan value from the right (when V is only slightly greater than L), the put option increases sharply in value. Any potential purchaser of the loan will be estimating the likelihood of further declines in the asset's value and also

FIGURE 2

Graphical Representation of a Bank's Potential Loss on a Secured Loan



Source: Merton (2009).

the increased volatility of the asset's price, which occurs as prices decline. In deciding how much to offer to purchase the risky loan, the potential purchaser will subtract the value of the put option from the terminal loan value (L), even though the loan may still be current (and therefore not impaired at all, as argued by the bank).

The large "bid-ask" spread, between what a rational bidder is prepared to pay for a risky loan with a loan-to-value ratio of 1, or less, is interpreted (incorrectly) by the bank as a decrease in the liquidity for its asset, not the actual decline in its underlying value. The bank rejects the markdown to fair value (the bid price) by claiming (1) it intends to hold the loan to maturity, and (2) the underlying cash flows from the asset have yet to be impaired in any way. But these are not valid reasons for failing to fair value a financial asset. When a company leverages its asset position with debt that is shorter term than the underlying asset, it loses control over whether the asset will be held to maturity; that right has been transferred to its creditors. They will make their own estimates of the asset's fair value and will likely subtract an additional discount to compensate for the possibility (from lack of disclosure) that they are dealing with a "lemons problem." Should the creditors' estimates of the bank's asset values be less than the bank's current liability position, the market for the bank's credit will disappear precipitously. The bank will complain about a sudden loss in liquidity, but the loss has been in the value of its asset holdings, not in the liquidity of markets. It is in the bank's self-interest to understand the fair value of its asset position better than its creditors, which many fail to do.

Fair Values are Often Not the Most Recent Market Price

A secured loan, illustrated above, is just one example of a thinly traded financial asset. Accountants could benefit from learning how to use contemporary market prices of similar assets to infer what price a particular asset would trade at should it trade at the next point of time. In the case of mortgage-based assets in the second half of 2008, trading may have decreased, but it was never zero. Nearly 1,000 securities traded each day, and these provided sufficient reference points to value all the asset holdings. A similar situation occurs for U.S.-based mutual funds holding equity assets that trade exclusively in overseas markets. The funds must, at the end of the U.S. trading day, calculate the fair values of these overseas equities for calculating the price at which it will buy or sell fund shares. The funds use estimates of the correlations between overseas and U.S. stock prices as well as the information in the closing prices of U.S.-traded equities to estimate the price at which the overseas equities *would have traded* in a transaction executed at the U.S. closing time.

As another example, U.S.-based funds of fixed income assets hold individual securities, most of which do not trade continually or even daily. Yet, at the end of each trading day, they must mark all their holdings to best estimates of their fair values. This calculation requires extensive statistical analysis of the correlations between the assets held by the funds and the assets that did trade at or near the end of the day. In effect, the funds use models similar to those used by the website zillow.com to estimate home prices based on actual sales of homes in the same neighborhood. The website has an algorithm that adjusts the actual transaction prices of homes in the neighborhood for differences between the traded homes and the nontraded homes. Financial institutions, such as Bear-Stearns, AIG, Merrill Lynch, and Lehman Brothers, that had not developed the capabilities to estimate the fair value of their asset holdings at the end of each trading day ended up being surprised by their insolvencies in 2007/08.

Fair Value of Compensation Packages

Complex fair value calculations also arise in executive compensation contracts, as illustrated by the "inducement portion" of the compensation plan offered to Carol Bartz to become CEO of Yahoo:

Shares Granted	Vesting Price	% Increase over Current Market Price
5,000,000	11.73	0%
1,666,667	17.60	50%
833,333	20.53	75%
833,333	23.46	100%
416,667	26.39	125%
416,667	29.33	150%
833,333	35.19	200%

Source: Yahoo Form 10-K for period ending 12/31/08. Note 17: 109.

Accounting scholars have studied compensation plans for decades. Has this research helped us to understand the properties of Bartz's inducement grant? Could we advise a member of the Yahoo! Compensation committee about the parameters of such a grant, such as the number and distribution of price points and the quantity of shares vested at each price point? How many accounting scholars or our students could calculate the fair value of this equity grant? If not, how can we give guidance to our graduates who we hope will become the auditors, CFOs, and board members of companies that issue such compensation contracts? Given all our interest in CEO compensation, this seems like it could be a fruitful area for our research and teaching.

I conducted some field research while at the 2010 AAA Annual Meeting to ascertain how we explain fair value measurements today to accounting students. I walked around the exhibition hall to examine Intermediate Accounting textbooks (a task that enabled me to skip my weight machine exercises in the hotel's fitness center later that day). In skimming the fair value chapter in each book, I saw detailed instructions on how to record asset value changes to fair values as required by current standards and regulations, but no coverage of how to estimate the fair values of risky financial assets. We have, implicitly, delegated to others the task of estimating fair values of complex securities and compensation contracts. Our students learn only how to enter the debits and credits, not the knowledge or expertise to estimate fair values. I doubt whether accounting professionals who have outsourced such a critical measurement can understand or audit the validity of the fair value estimates supplied to them.

I left the exhibition hall rather depressed. I imagined what a comparable situation would have been in physics in 1945, 40 years after the development of quantum mechanics. Would academic physicists still be teaching only Newtonian mechanics, and not instructing their students that this theory was not even approximately valid in certain important situations? Would biology professors in 1995 be instructing students only about 19th century Linnaean classification and Darwinian evolution, and ignoring a half-century of innovations in genetics and molecular biology?

I learned earlier this year that a major bank employs 500 accountants to mark its entire global portfolio of securities to fair value each day. The chief accounting officer told me that the bank cannot hire graduates from U.S. accounting departments for this task. The students do not know sufficient economics, mathematics, and statistics to perform the fair value calculations. This deficit is a direct result of accounting scholars not doing research on fair value measurement and therefore not being able to teach our students how to perform such calculations. And, unfortunately, measuring the fair value and risk of financial instruments is easy compared to estimating the fair value and risk of a company's real and intangible assets, including the capabilities and loyalty of its employees, its network of global suppliers, its customer relationships, and its new product pipeline.

VI. IMPLICATIONS FOR ACCOUNTING SCHOLARSHIP

To my young accounting scholar, I hope that you can begin to rectify this situation by attempting to increase the relevance and impact of your research, education, and teaching. Individual faculty scholarship, however, is only one component for closing the gap between academics in professional schools and their audience of current and future practitioners. Academic scholarship is a system of interconnected parts. Reform requires changes in all the system's elements including:

1. Doctoral training (Polzer et al. 2009).
2. Publication standards applied by journal editors and reviewers.
3. Faculty promotion criteria.
4. Textbook writing and educational curriculum.
5. School and program accreditation criteria.
6. University promotion standards that are sensitive to the different expectations between scholars in academic versus professional schools.

Over the past 40 years, these six elements have converged into a stable equilibrium of accounting scholarship characterized by a narrow set of acceptable research methods, all of which are clustered at the top of Roethlisberger's (1977) Knowledge Enterprise diagram. It will require great energy to break out of this equilibrium to allow and reward research methods that document, classify, and create new knowledge for emerging problems and opportunities in practice. That is not your job as a new Assistant Professor; it is a task that falls on tenured Full Professors in your profession to take on.

Innovation in Accounting Education

In my remaining time, let me address just component #4 in the system of academic scholarship: the implications of my remarks for your teaching and educational activities. First, take advantage of being in a professional school by choosing a portion of your teaching assignment to be in executive education programs. As you develop expertise in a particular field relevant to the professional practice of accounting and finance, you can test your ideas by teaching them to experienced professionals. These executives are sacrificing much of their time and their company's money to attend and benefit from your teaching. By sharing your research with them, you get excellent feedback along two dimensions:

1. Is the problem you are addressing relevant to their experience and practice?
2. Is the expertise you have developed for this problem relevant to their practice? Are you teaching them something worthwhile about the problem that they can productively apply to their company's situation?

If the answer to either of these two questions is "no," then you will certainly hear about their disappointment. But if knowledgeable professionals value learning about your proposed solution to a problem they face, you will have accomplished an important and highly satisfying teaching milestone. And you will not have to rely only on large sample surveys to get feedback on the relevance, applicability, and innovation of your proposed solution to an important problem in practice.

Second, mastering some of the new common body of knowledge for future accounting and finance professionals will require your students to receive extensive training in economics, finance, higher mathematics (including stochastic calculus), and multivariate statistics. This may be beyond what can be delivered in a core curriculum in undergraduate accounting or a two-year M.B.A. program. But certainly, as an innovative accounting scholar, you can develop teaching materials, and new courses for an elective track in valuing and measuring the risk for complex assets, strategies, and transactions. You will contribute to your profession by educating future practitioners in innovative, contemporary solutions to problems they will encounter during their careers.

Third, try to codify your innovations by developing new classroom teaching materials and innovative textbooks (or at least textbook chapters) that will allow your insights to be taught by your accounting academic colleagues to a much wider audience than your own students. This will enable your contributions to have a wider impact on future accounting practice.

VII. CONCLUSION

I hope you can now appreciate the many opportunities for productive scholarship during the next four or more decades of your academic career. Risk measurement and management is only one example of the many interesting and important issues being faced by accounting and finance professionals today. I am sure that the next four decades will contain as many new developments and challenges as the previous four have uncovered. I regret only that I cannot turn my own biological clock back 40+ years so I could join with you on the wonderful journey ahead.

While on this journey, work on important problems whose solutions will expand the common body of knowledge for the practice of accounting, finance, and management. Use research methods that help you understand the problems professionals face and attempt to develop innovative solutions that they can apply. Teach your insights and solutions to your students, and share them widely with your global academic colleagues. Remain excited about the research and teaching opportunities in your chosen academic field. Help to improve the world of professional practice.

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Investor Trading and the Post-Earnings-Announcement Drift

Benjamin C. Ayers
The University of Georgia
Oliver Zhen Li
The University of Arizona
P. Eric Yeung
The University of Georgia

ABSTRACT: We examine whether the two distinct post-earnings-announcement drifts associated with seasonal random-walk-based and analyst-based earnings surprises are attributable to the trading activities of distinct sets of investors. We predict and find that small (large) traders continue to trade in the direction of seasonal random-walk-based (analyst-based) earnings surprises after earnings announcements. We also find that when small (large) traders react more thoroughly to seasonal random-walk- (analyst-) based earnings surprises at the earnings announcements, the respective drift attenuates. Further evidence suggests that delayed small trades associated with random-walk-based surprises are consistent with small traders' failure to understand time-series properties of earnings, whereas delayed large trades associated with analyst-based surprises are more consistent with a longer price discovery process. We also find that the analyst-based drift has declined in recent years.

Keywords: *post-earnings-announcement drift; earnings expectations; market efficiency; small and large traders; investor sophistication.*

Data Availability: *All data are publicly available.*

JEL Classifications: *G14; M41.*

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I. INTRODUCTION

More than four decades of research on post-earnings-announcement drift consistently finds that stock prices tend to move in the direction of earnings surprises following earnings announcements. While the majority of prior studies focus on the drift associated with seasonal random-walk-based earnings surprises, recent studies document a drift associated with analyst-based earnings surprises that appears not only distinct from, but also even larger than the drift associated with seasonal random-walk-based earnings surprises (Livnat and Mendenhall 2006; Doyle et al. 2006). Despite the extensive literature on post-earnings-announcement drift, there is no consensus to date regarding the source(s) of either drift.[1]

Our study examines whether the distinct drifts are attributable, at least in part, to different identifiable subsets of investors who under-react to different forms of earnings innovations. Evidence on this issue is important for researchers and practitioners to understand the nature and the source of distinct drifts associated with seasonal random-walk-based earnings surprises and analyst-based earnings surprises. Indeed, Livnat and Mendenhall (2006, 180) argue that "[i]f researchers do not understand how the magnitude of the drift depends on the specification of earnings surprise, they stand little chance of understanding the nature of the anomaly."

We hypothesize that the two post-earnings-announcement drifts are attributable, at least in part, to distinct sets of investors (small and large traders) who use different earnings expectations models, but fail to fully react to earnings news. Specifically, if small traders use seasonal random-walk-based earnings expectations models but under-react to the seasonal random-walk-based earnings surprises, then we expect that post-announcement trading by small traders helps to explain the drift associated with seasonal random-walk-based earnings surprises (hereafter, RW drift). Similarly, if large traders base their earnings expectations on analyst forecasts but do not fully react to the analyst-based earnings surprises, then we anticipate that post-announcement trading by large traders helps to explain the drift associated with analyst-based earnings surprises (hereafter, AF drift).

Despite the intuitive appeal of our expectations, there is little evidence that links either drift to specific investors' trading. For example, although Battalio and Mendenhall (2005) and Bhattacharya (2001) find that small traders are more likely to trade on the RW earnings surprise around earnings announcements and large traders trade on the AF earnings surprise around earnings announcements, they do not link the RW or AF *drift* to trades by specific investor groups in the post-announcement period. Bartov et al. (2000) find that the RW drift decreases with the level of institutional ownership and conclude that institutional investors improve the efficiency with which RW earnings surprises are priced. However, they do not investigate the AF drift or link investor trades to the RW drift. More recently, Hirshleifer et al. (2008) investigate whether the RW drift is attributable to individual investors trading *contrary* to the RW earnings surprises at earnings announcements, which creates underpricing after good news and overpricing after bad news. Contrarian trading by individual investors is an alternative sufficient, but not *necessary* condition for the RW drift. Hirshleifer et al. (2008) find no evidence that individual investors trade contrary to RW earnings surprises, and offer three alternative explanations for the RW drift: the RW drift is an artifact of poor measurement of risk-adjusted returns; individual investors cause the RW drift but their proprietary sample from a single discount brokerage fails to detect this relation; or institutional investors cause the RW drift. In sum, the effect of distinct investor groups on the RW drift is far from settled and there is little explanation as to whom or what causes the AF drift.

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[1] For example, prior research conjectures that the drift associated with seasonal random-walk-based earnings surprises may be attributable to investor under-reaction (Bernard and Thomas 1989; 1990), a subset of naïve investors trading contrary to earnings news (Hirshleifer et al. 2008), other complex short- and long-term trading patterns by investors (Bartov et al. 2000; Ke and Ramalingegowda 2005), and mismeasurement of risk-adjusted returns (Ball 1992; Kothari 2001).

To build on prior research, we investigate the possibility that the RW and AF drifts could be explained by distinct groups of traders who under-react to differing earnings news. Using trade and quote data for trades executed from 1993 through 2005, we classify investors as small or large traders based on trade size, and use a buy-sell order imbalance metric to estimate large and small traders' net buying activities. We then examine small and large trades around and after earnings announcements and their relation to the post-earnings-announcement drifts.[2]

Results are consistent with our predictions. We find that only small traders systematically trade in the direction of the RW earnings surprises after earnings announcements. Likewise, we find that only large traders trade in the direction of the AF earnings surprises after earnings announcements. This evidence is consistent with distinct sets of traders explaining, at least in part, the two post-earnings-announcement drifts. Specifically, it suggests that small traders create upward (downward) price pressure on stocks in the post-announcement period for firms with good (bad) RW news. Likewise, it suggests that large traders create upward (downward) price pressure on stocks in the post-announcement period for firms with good (bad) AF news.

If the RW and AF drifts are manifested as an under-reaction to earnings news by small and large traders, respectively, then the more these traders act on the earnings news during the announcement period, the lower the subsequent drift should be. Consistent with this expectation, we find that when small traders trade more intensely in the direction of the RW earnings surprises *during the announcement period*, the magnitude of the subsequent RW drift is lower. Likewise, when large traders trade more intensely in the direction of the AF earnings surprises *during the announcement period*, the magnitude of the AF drift is lower.

Because large traders are considered *relatively* more sophisticated than are small traders (e.g., as evidenced by their more sophisticated earnings expectation model), the nature of the under-reaction by small and large traders and the related drifts is likely qualitatively different. We conduct several analyses to further explore and contrast the RW and AF drifts. First, we expect that the timing of post-announcement trades is a function of investors' *relative* sophistication. If large traders are relatively more sophisticated than small traders, we expect that large traders end their trading on AF earnings surprises more quickly than small traders end their trading on RW earnings surprises. Consistent with our expectation, we find that large traders trade more at the early stage of the post-announcement period, whereas small traders spread their trades throughout the 60-day post-announcement period.

Second, we examine small and large trades around one- to four-quarter-ahead earnings announcements using the methodology employed by Bernard and Thomas (1990). Consistent with small traders being less sophisticated, we find that small trades around subsequent earnings announcements are predictable based on lagged one- to four-quarter RW earnings surprises. Consistent with large traders being *relatively* more sophisticated and their under-reaction being short-lived, we find no evidence of large trades at earnings announcements being associated with prior AF earnings surprises. This evidence suggests that the RW drift, but not AF drift, is largely explained by small or unsophisticated traders' failure to recognize the time-series property of earnings.

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[2] Hirshleifer et al. (2008) use a proprietary sample of individual trades from a single discount brokerage, whereas we analyze a comprehensive sample of small and large trades using the New York Stock Exchange's Trade and Quote database. While our theory is not contingent on small traders being individual investors (i.e., our theory relies on small traders under-reacting to the less sophisticated, RW earnings surprises and large traders under-reacting to AF earnings surprises), we are able to reconcile our results to the results in Hirshleifer et al. (2008) using our small trade data. This evidence suggests that the individual investors in Hirshleifer et al. (2008) trade similarly to the small traders in this study's setting and that the differences in this study from Hirshleifer et al. (2008) stem more from the theories tested and research designs.

Third, we investigate whether the AF drift and post-announcement trades by large traders associated with the AF earnings surprise are more pronounced when the AF earnings surprise is more difficult to interpret. Prior research finds that the AF drift is greater when analysts' forecasts are more heterogeneous (Liang 2003), suggesting that investors have difficulty interpreting AF earnings surprises when analysts' forecasts are less correlated. Consistent with large traders requiring more post-earnings-announcement information to interpret earnings surprises when analysts' forecasts are more heterogeneous, we find that the AF drift and post-announcement trades by large traders associated with the AF earnings surprise are more pronounced when there is more heterogeneity among analysts' forecasts. This finding suggests that large traders' under-reaction and the related AF drift may be attributable, at least in part, to a longer price discovery process when earnings are more difficult to interpret.

Fourth, we investigate how post-earnings-announcement analyst forecast revisions affect large and small traders' post-announcement trading on earnings surprises. If large traders' under-reaction is corrected in the post-announcement period through subsequent price discovery, analyst forecast revisions may be one source of such information discovery (Gleason and Lee 2003).[3] We find that analyst forecast revisions are sluggish with respect to prior earnings information and that much of the post-announcement trading by large traders based on the AF earnings surprise is attributable to information in analyst forecast revisions that continue to be correlated with the AF earnings surprise well after the earnings announcement. This evidence further suggests a longer price discovery explanation for the AF drift with analyst forecast revisions serving as one source of information discovery.

Finally, we examine the magnitude of the RW and AF drifts and related post-announcement trading by small and large traders in more recent years. Results suggest that the magnitude of the AF drift decreases in more recent years (after 1999), while the RW drift does not change. This evidence is consistent with a more efficient price discovery process for information contained in the AF earnings surprise in more recent years as the AF drift may have received more attention. Given the relatively new discovery of and the little attention that the AF drift received during the earlier years in our sample, sophisticated investors may have not sufficiently exploited the AF drift during this period, which may explain the larger AF drift in earlier years.

This study makes the following contributions. First, we predict and find that small traders systematically trade in the direction of the RW earnings surprise after earnings announcements, whereas large traders' trading during the post-earnings-announcement period is in the direction of the AF earnings surprise. This evidence suggests that the RW and AF drifts are attributable, at least in part, to distinct sets of investors who use different earnings expectation models. Second, to our knowledge, this is the first study to link *announcement period trading* by distinct groups of investors to the *RW* and *AF drifts*. These findings are important in understanding the respective drifts and the investors who influence the size and type of the drifts. Third, we provide evidence that the two drifts are qualitatively different. The RW drift is largely explained by small traders' failure to understand the time-series property of earnings and therefore represents investor naiveté. In contrast, the AF drift may be explained by a longer price discovery process by large traders that highly correlates with subsequent analyst forecast revisions and increases with analysts' forecast heterogeneity. Finally, we find that the AF drift and related large trades during the post-announcement period have decreased in more recent years as the AF drift may have received more attention. In sum, we conclude that the two drifts are quite different despite both being manifested as an investor under-reaction to earnings news.

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[3] Gleason and Lee (2003) find drift associated with analyst forecast revisions, consistent with a price discovery process. Because the price discovery process is *systematically correlated* with the AF earnings surprise and the AF drift is mitigated when large trades react more strongly to the AF earnings surprise during the announcement period, we characterize large traders as under-reacting to the AF earnings surprise.

Section II develops hypotheses. Section III describes how we classify trades into buys and sells and how we partition small and large traders. Section IV presents our sample selection and descriptive statistics, and Section V presents the results of our hypotheses tests. Section VI provides evidence of additional differences between the AF and RW drifts. Section VII summarizes and concludes.

II. HYPOTHESIS DEVELOPMENT

Investors' Post-Announcement Trading

To date, there is no consensus regarding who or what causes either drift, random-walk-based or analyst-based. While prior studies have not investigated the source(s) of the AF drift (given its relatively new discovery), recent studies have begun to focus on how different investor groups influence the RW drift. Bartov et al. (2000) find that the RW drift decreases with the level of institutional ownership and conclude that institutional investors improve on the earnings-processing problems that cause the RW drift. Their finding implies that higher individual ownership exacerbates the RW drift. Similarly, Ke and Ramalingegowda (2005) find that quarterly changes in *transient* institutional ownership in the direction of RW earnings surprise (e.g., an increase in ownership after good news) are associated with lower RW drift. This evidence suggests that more aggressive trading by transient institutional investors at earnings announcements negates mispricing associated with RW earnings surprises.

More recently, Hirshleifer et al. (2008) use individual trading data to investigate whether naïve individual investors trade *contrary* to the RW earnings surprises at earnings announcements, which creates underpricing after good RW news and overpricing after bad RW news. Implicit in the "contrarian trading" hypothesis is the presence of a group of informed or more sophisticated traders who attempt to move prices based on the RW earnings surprises and a second group of naïve individual investors who trade contrary to the RW earnings surprises. Hirshleifer et al. (2008) use a proprietary sample of individual trades from a single discount brokerage firm and find no evidence that individual investors trade contrary to RW earnings surprises.[4]

To build on prior research, we investigate an alternative explanation for the RW drift (and AF drift). We hypothesize that the two distinct post-earnings-announcement drifts are attributable, at least in part, to distinct sets of investors (small and large traders) who use different earnings expectation models but fail to fully react to earnings news. Our expectations differ from Hirshleifer et al. (2008) in that our reasoning is based on two different investor groups (small and large traders) trading on two different earnings signals (RW and AF earnings surprises) with both investor groups under-reacting to their respective earnings signals, as opposed to two different investor groups trading on one signal (RW earnings surprises) with one naïve group trading contrary to the signal.

We base our hypothesis on four streams of prior research: (1) findings that during the *earnings-announcement period*, small traders react to RW earnings surprises, while large traders tend to react to more accurate AF earnings surprises,[5] (2) asset-pricing models that suggest that equilibrium prices aggregate different information sets of all investors, (3) behavioral evidence that investors tend to under-react to news, and (4) evidence that arbitrage activities are often limited. Collectively, these factors create a sufficient condition to generate two distinct drifts.

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[4] Shanthikumar (2004) examines small and large trades with the NYSE Trades and Quotations data and finds inconclusive evidence regarding the cause of the post-announcement earnings drift. Given the relatively new discovery of the AF drift, Shanthikumar (2004) does not consider the joint effect of RW and AF earnings surprises on investor trades and their relation with the AF and RW drifts.

[5] While prior research shows that analysts' quarterly forecasts are more accurate than earnings predicted by time-series models, Bradshaw et al. (2010) show that simple random-walk forecasts are more accurate than analyst forecasts over longer forecast horizons and for firms that are smaller, younger, or have limited analyst following. Analysts' superiority

Bhattacharya (2001) studies the trading volume of large and small traders during earnings-announcement periods and finds that small traders' earnings expectations resemble a seasonal random-walk. Battalio and Mendenhall (2005) employ order imbalance measures and find that around earnings announcements, small traders respond to RW earnings surprises while large traders respond to AF earnings surprises. Mikhail et al. (2007) find that, while large traders trade in the direction of analyst stock recommendations and earnings forecast revisions, small traders do not. Collectively, these findings suggest that large traders use a more sophisticated earnings expectation, consistent with large traders likely being wealthier and more informed than small traders (Easley and O'Hara 1987; Lee 1992). The implication of these findings is that different investor groups (i.e., small and large traders) systematically form different earnings expectations and trade accordingly.[6,7]

Prior equilibrium models suggest that investors with differing beliefs can impact prices (Grossman 1976; Grossman and Stiglitz 1980; Admati 1985; Kandel and Pearson 1995; Shleifer and Vishny 1997). These models suggest that, in equilibrium, stock prices reflect the weighted average beliefs of different investor groups, with the risk-bearing capacity of each investor group determining its relative weight. These models also suggest that investor groups with differing earnings expectations (e.g., small traders who form their earnings expectation and trade based on RW earnings surprises and large traders who form their earnings expectation and trade based on AF earnings surprises) can impact prices. Consistent with this implication, Walther (1997) finds that stock returns reflect both RW earnings surprises and AF earnings surprises.

Prior behavioral research has suggested that market participants generally underestimate the implications of current earnings for future earnings. For example, Maines and Hand (1996) and Calegari and Fargher (1997) find that both undergraduate business and M.B.A. students seem to underestimate the auto-regressive component of the seasonal changes in quarterly earnings in the process of forecasting earnings. Stevens and Williams (2004) find that undergraduate business students under-react to both positive and negative news. Likewise, Mendenhall (1991), Ali et al. (1992), Abarbanell and Bernard (1992), and Abarbanell and Bushee (1997) find that analysts underestimate the persistence of earnings surprises in revising their earnings forecasts. In addition, several studies (e.g., Bushee 2001; Ke and Petroni 2004; Bradshaw et al. 2004; Callen et al. 2005) find that institutional investors have difficulty interpreting financial information.[8]

Arbitrage activities can potentially reduce the magnitude of the drifts. However, prior research shows that arbitrage forces can be quite limited in financial markets (De Long et al. 1990a, 1990b; Shleifer and Vishny 1997). A maintained assumption of our analysis is that arbitrageurs do not

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is also less prevalent when analysts forecast large changes in earnings per share. These findings suggest that under certain conditions, sophisticated (large) traders should focus on random-walk-based earnings surprises instead of analyst-based earnings surprises. If this is the case, then it would bias against finding results consistent with our expectation for differential trading for large and small traders.

[6] Use of random-walk-based expectations by small traders is likely, in part, attributable to their having less access to analyst forecasts. Consistent with this explanation, we find in tests described later that small traders' reliance on analyst forecasts increases in more recent sample years as analyst forecasts are more accessible (e.g., on the Internet).

[7] None of these prior studies link small or large trades to RW or AF earnings surprises in the *post-announcement* period, leaving the questions of which investors or trading patterns cause the respective drifts open.

[8] Bushee (2001) provides evidence that some institutional investors over- (under-) weight short- (long-) term earnings in their valuation models. Likewise, Ke and Petroni (2004) find that even though sophisticated institutional investors anticipate the break of a string of consecutive earnings increases, their selling activities lag behind the decline of stock prices by one to two quarters. In addition, Bradshaw et al. (2004) and Callen et al. (2005) find that institutional investors suffer from biases in interpreting foreign earnings.

completely eliminate under-reaction, because if arbitrage activities quickly correct initial under-reaction to earnings news, these post-announcement drifts would not occur.

Overall, the implication of these four streams of research is that the RW and AF drifts could be explained by distinct groups of investors that form their earnings expectations differently, who both impact prices and systematically under-react to earnings news. Because small traders are more likely to use RW earnings models to form their earnings expectations, we expect that trading by small traders in the post-announcement period primarily contributes to the RW drift. Likewise, because large traders are more apt to base their earnings expectations on analyst forecasts, we anticipate that their trading contributes more to the AF drift if they likewise fail to fully react to earnings surprises. We formulate the following hypotheses related to the post-earnings-announcement trading behavior of small and large traders:

H1: Small traders are more likely to trade shares in the direction of seasonal random-walk-based earnings surprises after earnings announcements.

H2: Large traders are more likely to trade shares in the direction of analyst-based earnings surprises after earnings announcements.

Recent research questions whether small traders represent individuals or instead institutions that split their trades into smaller trades (Barclay and Warner 1993; Chakravarty 2001; Campbell et al. 2005), whereas other studies present evidence consistent with small traders representing individual investors (Easley and O'Hara 1987; Lee 1992; De Franco et al. 2007; Bhattacharya et al. 2007; Ayers et al. 2008). It is important to note that this study's hypotheses are not contingent on small traders being individual investors and large traders being institutions. Instead, our expectations rely solely on small traders under-reacting to the less sophisticated RW earnings surprises and large traders under-reacting to AF earnings surprises. To the extent that both small traders and large traders represent the same traders (e.g., institutions that split their trades into small trades but use analyst forecasts to create their earnings expectations), this should bias against finding evidence supporting our hypotheses that predict trading differences between small and large traders.

Effect of Earnings-Announcement Period Trading on Subsequent Drift

If small and large traders' activities help explain the RW and AF drifts, respectively, then we expect a relation between the two drifts and the respective trading intensity by small and large traders *around earnings announcements*. Specifically, if investors assimilate the earnings news more thoroughly and thus trade more intensely around earnings announcements on RW and AF earnings surprises, then the magnitude of the respective RW and AF drifts should be lower. Since we hypothesize that small traders contribute to the RW drift, we expect that the more they trade at the announcement period on RW earnings surprises, the lower the RW drift. Likewise, we expect that the more large traders trade at the announcement period on AF earnings surprises, the lower the AF drift:

H3: The more intense the earnings-announcement period trading by small traders on seasonal random-walk-based earnings surprises, the smaller the post-earnings-announcement drift associated with seasonal random-walk-based earnings surprises.

H4: The more intense the earnings-announcement period trading by large traders on analyst-based earnings surprises, the smaller the post-earnings-announcement drift associated with analyst-based earnings surprises.

III. BUY-SELL ORDER IMBALANCE FOR SMALL AND LARGE TRADERS

We obtain stock quotes and investor trade data from the New York Stock Exchange's Trade and Quote (TAQ) database for years 1993, the year the TAQ database begins, through 2005. Using the Lee and Ready (1991) and Lee (1992) algorithm, we classify trades into buyer-initiated or seller-initiated trades. While the number of shares bought equals the number of shares sold in a transaction, the Lee (1992) algorithm identifies the likelihood that a transaction is buyer-initiated or seller-initiated.[9]

Specifically, we compare traded prices with quotes that are at least five seconds earlier. If the traded price is above the mid-point of the bid-ask spread, then we define the trade as a buy (*BUY*). If the traded price is below the mid-point of the bid-ask spread, then we define the trade as a sell (*SELL*). For day *k*, we add up all the buys $\Sigma_{n=1}^{M} BUY_{im}$ and all the sells $\Sigma_{n=1}^{N} SELL_{in}$, where M is the total number of buys on Day *k* and N is the total number of sells on Day *k*. We then compute daily buy-sell order imbalance (i.e., net-buy) BMS_i ($\Sigma_{m=1}^{M} BUY_{im} - \Sigma_{n=1}^{N} SELL_{in}$). A positive (negative) BMS_i indicates net-buying (net-selling) on Day *k*.

Because we are interested in *excess* trading activities, we subtract *normal* daily net-buy from the daily net-buy measure during the event period. We measure normal daily net-buy during the control period, a 40-day period from Day −45 to Day −6 relative to the earnings announcement Day *i*.[10] Specifically, the normal daily net-buy relative to Day *i* is the average daily net-buy computed as:

$$NBMS_i = \sum_{k \in [-45,-6]} BMS_{i,k}/T \tag{1}$$

where T is the total number of days with data available for *BMS*. Our daily excess net-buy measure is defined as $BMS_i - NBMS_i$.

Finally, to facilitate cross-sectional comparison of our daily excess net-buy measure, we scale it by the normal daily trading volume (i.e., daily trading volume during the control period). Daily trading volume, BPS_i, is defined as daily *BUY* plus daily *SELL* (i.e., $\Sigma_{m=1}^{M} BUY_{im} + \Sigma_{n=1}^{N} SELL_{in}$), and the normal daily trading volume relative to Day *i* is the average daily buy plus sell:

$$NBPS_i = \sum_{k \in [-45,-6]} BPS_{i,k}/T \tag{2}$$

where T is the total number of days with data available for *BPS*.[11] The excess net-buy on Day *k* during the event period (relative to the earnings announcement Day *i*) is therefore defined as net-buy in excess of the control period net-buy, scaled by the control period volume:

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[9] Based on Lee (1992), a transaction is classified as buyer- (seller-) initiated if a buyer (seller) demands immediate execution (i.e., a market order). Generally there are three types of orders: market orders, limit orders, and standing orders. A market order demands immediate execution. A limit order is an order to be executed when a pre-specified price is attained. A standing order is an order to buy or sell shares at the best available price over a certain period of time, during which a broker has to use discretion. After the opening trade, a trade occurs only when a market order arrives. If a market order to buy is filled by a limit order to sell, the trade is classified as a buyer-initiated trade. If a market order to sell is filled by a limit order to buy, the trade is classified as a seller-initiated trade. Sometimes, the size of a market order and the size of a limit order are not equal. If one large market order to buy (sell) is filled by several small limit orders to sell (buy) (and possibly partially filled by the specialist), then the trade is classified as one large buyer- (seller-) initiated trade. If several small market orders to buy (sell) are filled by one large limit order to sell (buy) (and possibly partially filled by the specialist), then the trades are classified as several buyer- (seller-) initiated trades. Lee and Radhakrishna (2000), using the Trades, Orders, Reports, and Quotes database that contains information on trade directions and trader identities, find that while few (6 percent) of the total market orders are split up in execution, a much larger portion (24 percent) of the total market orders is batched in execution. However, despite the prevalence of order batching, trade size is still highly effective in separating large traders and small traders.

[10] Results are similar if we do not base our test on *excess* net-buys—i.e., if we do not create our measure of abnormal daily net-buys by subtracting the normal daily net-buys during the control period.

[11] We require T to be at least 20 to compute $NBPS_i$ and $NBMS_i$.

$$EXBMS_{i,k} = \frac{BMS_{i,k} - NBMS_i}{NBPS_i}. \tag{3}$$

This measure captures the excess net-buying activities on Day k. Once we have a measure of daily excess net-buy, we calculate the average excess net-buy during an event window $[k_1, k_2]$ (relative to the earnings announcement Day i) as $\Sigma_{k=k_1}^{k_2} EXBMS_{i,k} / (k_2 - k_1 + 1)$.

To analyze the trading behavior of small and large traders, we compute excess net-buy for small trades and large trades separately.[12] Specifically, if the dollar value of a round lot trade is below \$5,000, then we classify the trade as a small trade. If the dollar value of a round lot trade is above \$30,000, then we classify the trade as a large trade. Note that there are no exact cut-offs for defining small and large trades. The \$5,000 and \$30,000 cut-offs are used here to distinguish small and large traders that prior research suggests form earnings expectations based on RW and AF earnings surprises, respectively. We use a buffer of \$25,000 between the \$5,000 cut-off for small trades and the \$30,000 cut-off for large trades to reduce the ambiguity that a trade is initiated by a small or large trader (Lee and Radhakrishna 2000).[13]

IV. SAMPLE AND DESCRIPTIVE STATISTICS

Sample Selection

We obtain actual quarterly earnings, analyst forecasts of quarterly earnings, and earnings announcement dates from the I/B/E/S unadjusted file for the period 1993–2005.[14] We also require firms to have I/B/E/S earnings for the same quarter in the prior year to calculate RW earnings surprise. We define RW earnings surprise (*RW*) as:

$$RW = (EPS_t - EPS_{t-4})/P_{t-1} \tag{4}$$

where EPS_t is the I/B/E/S actual earnings per share (EPS) for quarter t, EPS_{t-4} is the I/B/E/S actual EPS for quarter t–4, and P_{t-1} is stock price at the beginning of quarter t.[15,16]

Similar to Equation (4), we define AF earnings surprise (*AF*) as the I/B/E/S actual EPS for quarter t minus the analyst forecast for quarter t, deflated by beginning of quarter t stock price in Compustat. We use the single-most recent forecast made by the timeliest analyst(s) prior to the earnings announcement date as our analyst forecast.[17] When there is more than one analyst forecast on the most recent day, we take the mean of these forecasts. Consistent with prior studies, we convert *RW* and *AF* into earnings surprise deciles based on the magnitude of earnings surprises in the population in the same quarter. Similar to Livnat and Mendenhall (2006), we focus on a 60-day post-earnings-announcement period and require firms to have stock return data from the CRSP Daily Stock file during the period [−1, +65] surrounding earnings announcement dates. We obtain 129,215 observations based on these selection criteria. Finally, we merge this data with excess net-buy measures and require no missing excess net-buy measures for both the [−1, +1] and [+6, +65] periods following earnings announcements.[18] For regression analyses, we delete observations in the top or bottom 1 percentile in the distributions of excess net-buy measures. Our final sample includes 73,469 observations for 5,661 firms.[19]

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[12] Battalio and Mendenhall (2005) classify trade size by number of shares. We classify traders based on the dollar amount involved, because this is a relatively more direct measure of investor wealth (Bhattacharya 2001; Bhattacharya et al. 2007).

[13] As a sensitivity analysis, we use alternative cut-offs such as \$10,000 and \$15,000 for small trades and \$20,000 and \$25,000 for large trades. Results are robust to these alternative cut-offs.

[14] We use earnings announcements dates from I/B/E/S because we require analyst forecasts issued prior to I/B/E/S earnings announcement dates. The earnings announcement dates in I/B/E/S differ from Compustat by no more than one calendar day.

[15] While prior research uses Compustat actual earnings to calculate RW earnings surprises, we use I/B/E/S actual earnings to be consistent with the definition of AF earnings surprises. Livnat and Mendenhall (2006) demonstrate that using I/B/E/S actual earnings does not significantly alter the RW drift. Our results are similar when we use Compustat actual earnings to calculate RW earnings surprises.

[16] Following Livnat and Mendenhall (2006), we define *RW* and *AF* so that they only differ in expected earnings (i.e., they have the same reported actual earnings and same deflator). In sensitivity analyses, we use the standard deviation of prior RW surprises over the past eight quarters as the deflator for RW (Bernard and Thomas 1990) and obtain similar results.

[17] Brown and Caylor (2005) argue that the most recent forecast is a better proxy for expected earnings than the consensus forecast for two reasons. First, using the most recent forecast mitigates the effects of pre-announcements. Second, evidence suggests that the most recent forecast is more closely related to the stock price reaction to earnings announcements (Brown and Kim 1991). When we use the consensus analyst forecast as the benchmark for our analysis, we obtain qualitatively similar results.

Descriptive Statistics for the Earnings Announcements Period

Table 1 displays abnormal returns and average excess net-buy for small and large traders during the three-day [−1, +1] earnings-announcement period, for quintiles of RW and AF earnings surprises. Panel A reports the magnitude of the average value of three-day cumulative abnormal returns for the 25 portfolios formed by the RW and AF earnings surprises quintiles. Within each AF quintile, abnormal returns generally increase in RW quintiles, and within each RW quintile, abnormal returns generally increase in AF quintiles, consistent with Walther (1997).

Panel B of Table 1 shows the mean values of three-day average excess net-buy for small traders around the earnings-announcement period for the 25 portfolios formed by RW and AF quintiles. Within each AF quintile, small traders' excess net-buy generally increases in RW quintiles. On the other hand, within each RW quintile, the pattern with respect to AF quintiles is less clear. Consistent with Bhattacharya (2001) and Battalio and Mendenhall (2005), our data suggest that small traders react more to RW earnings surprises during the announcement period.

Panel C of Table 1 shows the mean value of three-day average excess net-buy for large traders around the earnings-announcement period. Within each RW quintile, large traders' excess net-buy generally increases in AF quintiles. However, within each AF quintile, we do not observe any systematic pattern between large traders' excess net-buy and RW quintiles. In sum, data indicate that large traders' earnings-announcement period trading responds only to AF earnings surprises, consistent with Battalio and Mendenhall (2005).[20]

Descriptive Statistics for the Post-Earnings-Announcement Period

Table 2 provides statistics on the 60-day [+6, +65] cumulative abnormal returns during the post-earnings-announcement period, and small and large trader excess net-buy for RW and AF quintiles. Panel A reports the average 60-day cumulative abnormal returns for the 25 portfolios sorted on RW and AF earnings surprises. Overall, the average abnormal returns during the post-earnings-announcement period generally increase both in AF and RW earnings surprises, although the results are not monotonic within quintiles. Regarding the magnitudes of the two drifts, we find that the spread in abnormal returns between *AF*5 and *AF*1 is 4.8 percent, while the spread between

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[18] Because our focus is on trading during the *post*-earnings-announcement period, we do not analyze trading data during the [+2, +5] window to avoid ambiguity as prior research (Morse 1981; Bamber 1987) finds that volume reactions extend up to day +5.

[19] To determine if results are robust to less stringent sample criteria, we relax the requirement that each firm-quarter has 60 days of post-announcement trading data. Specifically, we compute the daily average post-announcement buy-sell order imbalance for firm-quarters that have at least 20 days of buy-sell order imbalance data during the [+6, +65] post-announcement period. Relaxing this requirement increases the total number of observations in our main analysis to 94,917 (out of 112,876 observations after we require TAQ data for the 40-day control period and 3-day announcement period) and yields similar results.

[20] We note that the contrarian trading hypothesis in Hirshleifer et al. (2008) predicts that small traders trade contrary to RW earnings surprises (not AF earnings surprise) during the announcement period. We find no evidence of contrarian trading by either small or large traders during the announcement period. Thus, like Hirshleifer et al. (2008), our evidence does not support the contrarian trading hypothesis.

TABLE 1

Abnormal Returns and Excess Net-Buy for Small and Large Traders during [−1, +1] Earnings-Announcement Period

Panel A: Cumulative Abnormal Returns during [−1, +1]

	*RW*1	*RW*2	*RW*3	*RW*4	*RW*5	Average
*AF*1	−0.030	−0.027	−0.023	−0.017	−0.017	−0.026
*AF*2	−0.006	−0.011	−0.009	−0.010	−0.003	−0.009
*AF*3	−0.002	0.005	0.008	0.008	0.001	0.006
*AF*4	0.006	0.013	0.020	0.022	0.015	0.017
*AF*5	0.017	0.020	0.021	0.030	0.032	0.027
Average	−0.010	−0.004	0.003	0.011	0.015	

Panel B: Average Excess Net-Buy for Small Trades during [−1, +1]

	*RW*1	*RW*2	*RW*3	*RW*4	*RW*5	Average
*AF*1	0.008	0.033	0.044	0.027	0.062	0.026
*AF*2	0.017	0.034	0.049	0.049	0.052	0.040
*AF*3	0.038	0.033	0.057	0.075	0.061	0.055
*AF*4	0.021	0.034	0.045	0.053	0.082	0.050
*AF*5	0.031	0.058	0.043	0.050	0.082	0.059
Average	0.018	0.034	0.050	0.056	0.074	

Panel C: Average Excess Net-Buy for Large Trades during [−1, +1]

	*RW*1	*RW*2	*RW*3	*RW*4	*RW*5	Average
*AF*1	−0.012	−0.029	−0.012	0.001	−0.031	−0.018
*AF*2	−0.005	0.006	0.000	−0.001	0.051	0.005
*AF*3	0.041	0.024	0.041	0.034	0.029	0.034
*AF*4	0.050	0.044	0.078	0.039	0.037	0.049
*AF*5	0.069	0.061	0.057	0.098	0.081	0.078
Average	0.017	0.015	0.032	0.037	0.050	

Panel A shows cumulative abnormal return during [−1, +1] window around earnings announcements for seasonal random-walk earnings surprise quintiles (*RW*1–5) and for analyst-based earnings surprise quintiles (*AF*1–5). Abnormal return during the earnings-announcement period is defined as the firm return in excess of the corresponding Fama-French size and book-to-market six-portfolio benchmark return. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*–4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*.

Panel B shows average excess net-buy for small trades during [−1, +1] window around earnings announcements for seasonal random-walk earnings surprise quintiles (*RW*1–5) and for analyst-based earnings surprise quintiles (*AF*1–5). Small trade is defined as a trade at maximum $5,000.

Panel C shows average excess net-buy for large trades during [−1, +1] window around earnings announcements for seasonal random-walk earnings surprise quintiles (*RW*1–5) and for analyst-based earnings surprise quintiles (*AF*1–5). Large trade is defined as a trade at minimum $30,000.

*RW*5 and *RW*1 is 4.3 percent. The spreads are not statistically different ($p = 0.60$), but they are comparable to those reported by Livnat and Mendenhall (2006), who find a spread of 4.9 percent between the top and the bottom AF deciles, and 3.8 percent between the top and the bottom RW earnings surprise deciles in their sample.

TABLE 2

Abnormal Returns and Excess Net-Buy for Small and Large Trades during [+6, +65] Post-Earnings-Announcement Period

Panel A: Cumulative Abnormal Returns during [+6, +65]

	*RW*1	*RW*2	*RW*3	*RW*4	*RW*5	Average
*AF*1	−0.030	−0.017	−0.005	−0.001	−0.015	−0.023
*AF*2	−0.030	−0.033	−0.026	−0.010	−0.020	−0.025
*AF*3	−0.022	−0.019	−0.004	−0.008	0.011	−0.008
*AF*4	−0.009	−0.023	0.004	0.024	0.012	0.005
*AF*5	0.004	−0.020	0.013	0.012	0.053	0.025
Average	−0.021	−0.024	−0.008	0.005	0.022	

Panel B: Average Excess Net-Buy for Small Trades during [+6, +65]

	*RW*1	*RW*2	*RW*3	*RW*4	*RW*5	Average
*AF*1	0.006	0.013	0.020	0.021	0.025	0.012
*AF*2	0.010	0.012	0.023	0.024	0.018	0.017
*AF*3	−0.008	0.004	0.020	0.028	0.035	0.018
*AF*4	−0.002	0.005	0.011	0.023	0.030	0.016
*AF*5	0.007	0.011	0.008	0.016	0.029	0.019
Average	0.005	0.009	0.018	0.023	0.028	

Panel C: Average Excess Net-Buy for Large Trades during [+6, +65]

	*RW*1	*RW*2	*RW*3	*RW*4	*RW*5	Average
*AF*1	0.025	0.002	−0.009	0.001	−0.002	0.011
*AF*2	0.020	0.005	0.004	−0.005	−0.003	0.004
*AF*3	0.022	0.013	0.007	0.007	−0.006	0.008
*AF*4	0.025	0.014	0.018	0.012	0.004	0.013
*AF*5	0.013	0.016	0.025	0.019	0.014	0.015
Average	0.022	0.008	0.008	0.007	0.006	

Panel A shows cumulative abnormal return during [+6, +65] window following earnings announcements for seasonal random-walk earnings surprise quintiles (*RW*1–5) and for analyst-based earnings surprise quintiles (*AF*1–5). Abnormal return during the post-earnings-announcement period is defined as the firm return in excess of the corresponding Fama-French size and book-to-market six-portfolio benchmark return. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*–4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*.

Panel B shows average excess net-buy for small trades during [+6, +65] window following earnings announcements for seasonal random-walk earnings surprise quintiles (*RW*1–5) and for analyst-based earnings surprise quintiles (*AF*1–5). Small trade is defined as a trade at maximum $5,000.

Panel C shows average excess net-buy for large trades during [+6, +65] window following earnings announcements for seasonal random-walk earnings surprise quintiles (*RW*1–5) and for analyst-based earnings surprise quintiles (*AF*1–5). Large trade is defined as a trade at minimum $30,000.

Panel B of Table 2 presents the mean value of average excess net-buy for small trades for the 60-day post-earnings-announcement period. Results suggest that, within each AF quintile, small trader excess net-buy generally increases in RW quintiles. Thus, consistent with H1, small traders continue to trade on information in RW earnings surprises in the post-announcement period. We also find that small traders' excess net-buys decline in AF quintiles in the second and third RW quintiles. Therefore, it appears that, while small traders continue to trade in the direction of RW earnings surprises, they also trade in the opposite direction of AF earnings surprises.

Panel C of Table 2 focuses on the average excess net-buy for large traders during the 60-day post-earnings-announcement period. In the second, third, fourth and fifth quintiles of the RW earnings surprises, large traders' excess net-buys generally increase in AF quintiles. Thus, consistent with H2, large traders continue to trade on information in AF earnings surprises in the post-announcement period. In the first, second, third, and fourth AF quintiles, large traders' excess net-buys generally decrease in RW quintiles. Therefore, it appears that, while large traders continue to trade in the direction of AF earnings surprises, they also trade in the opposite direction of RW earnings surprises.

V. RESULTS OF HYPOTHESES TESTS

Trading during the Earnings-Announcement Period

We first examine trading during the earnings-announcement period. Specifically, to estimate Equation (5), we regress cumulative abnormal returns (*CAR*) or average excess net-buy (*EXBMS*) for small or large traders during the three-day (i.e., $[-1, +1]$) earnings-announcement period on the decile ranks of the seasonal random-walk-based earnings surprises (*RW*) and the decile ranks of the analyst-based earnings surprises (*AF*). Following Bernard and Thomas (1989), we scale *RW* and *AF* to be between 0 and 1 in all regression analyses:

$$CAR_{it}/DRIFT_{it}/EXBMS\_Small_{it}/EXBMS\_Large_{it} = \beta_0 + \beta_1 RW_{it} + \beta_2 AF_{it} + \varepsilon_{1it}. \tag{5}$$

We report regression results estimating Equation (5) in Panel A of Table 3. All t-statistics in this table and subsequent tables are adjusted for clustering by firm (Rogers 1993). When the three-day *CAR* is the dependent variable, the coefficients on the seasonal random-walk earnings surprise, *RW*, and the analyst-based earnings surprise, *AF*, are positive and significant ($\beta_1 = 0.011$, $t = 10.59$, $p < 0.01$; $\beta_2 = 0.056$, $t = 46.46$, $p < 0.01$).[21] An F-test indicates that β_2 is significantly greater than β_1 ($p < 0.01$), suggesting that, while the market as a whole reacts to both the RW earnings surprises and the AF earnings surprises (Walther 1997), it reacts more intensely to AF earnings surprises.[22]

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[21] *RW* and *AF* are not highly correlated (correlation = 0.34), which suggests that the "earnings news" conveyed by the same reported number can generate significant differential implications if investors have different earnings expectation models. Similarly, regression diagnostics do not suggest that multicollinearity is an issue. The variance inflation factor (VIF) for Equation (5) is 1.13, well below the conventional value of 10, indicating a multicollinearity problem. In supplemental analysis, we analyze small and large trades for firms with no I/B/E/S analyst following (i.e., limited or no analyst following). We find that during the earnings-announcement period small traders react to RW earnings surprises, while large trades are *not* associated with RW earnings surprises. We also find that after earnings announcements, small trades are positively associated with RW earnings surprises, while large trades are negatively associated with RW earnings surprises. These results indicate that small traders react to RW earnings surprises at the earnings announcements and continue to trade in the direction of RW earnings surprises after the earnings announcements. Large traders, however, are more sophisticated, potentially understand that RW earnings surprises are inferior signals, and update their expectations based on other sources of information, such as market, firm, and industry news events. Within the sample of firms without I/B/E/S analyst following, these results also hold for firms in the top tercile of market value and top tercile of large trader trading volume, which suggests that transaction costs do not explain large traders' lack of trading on RW earnings surprises.

[22] In sensitivity analyses, we analyze those firms with conflicting signs of RW and AF earnings surprises. We find that 18.8 percent of the time a firm meets/beats analyst forecasts but fails to beat earnings of the same quarter in the prior year, and 12.5 percent of the time a firm meets/beats prior earnings but reports earnings lower than analysts' expectations. We find that firms with conflicting signs of RW and AF earnings surprises are not significantly different in size, trading volume, expected growth, number of analyst following, or analysts' forecasts dispersion. Finally, we find that the RW and AF drifts and related trading patterns are not significantly affected by whether the sign of RW and AF earnings surprises agree or disagree.

TABLE 3

Regressions of Abnormal Returns and Excess Net-Buy for Small and Large Trades

Panel A: Three-Day Announcement Period

	CAR	**Small Trades**	**Large Trades**
Intercept	−0.031	0.011	−0.026
	(−43.31) ***	(4.08) ***	(−4.38)***
RW	0.011	0.055	0.005
	(10.59) ***	(13.84) ***	(0.55)
AF	0.056	0.016	0.106
	(46.46) ***	(3.81) ***	(11.41)***
R^2 (%)	5.78	0.38	0.26
n	73,469	73,469	73,468

Panel B: 60-Day Post-Announcement Period

	DRIFT	**Small Trades**	**Large Trades**
Intercept	−0.047	0.004	0.012
	(−13.29) ***	(3.46) ***	(5.84)***
RW	0.040	0.030	−0.018
	(6.73) ***	(15.21) ***	(−5.77)***
AF	0.043	−0.005	0.015
	(7.47) ***	(−2.58) ***	(4.30)***
R^2 (%)	0.26	0.28	0.16
n	73,469	73,469	73,469

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*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.

All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

Table 3 shows regression of cumulative abnormal return, average excess net-buy of small trades, and average excess net-buy of large trades during [−1, +1] earnings announcement window and during [+6, +65] window following earnings announcements. Abnormal return during the post-earnings-announcement period is defined as the firm return in excess of the corresponding Fama-French size and book-to-market six-portfolio benchmark return. *RW* is the decile rank of seasonal random-walk earnings surprise converted to [0, 1], and *AF* is the decile rank of analyst-based earnings surprise converted to [0, 1]. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*–4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*. Small trade is defined as a trade at maximum $5,000. Large trade is defined as a trade at minimum $30,000.

When the small traders' excess net-buy (*EXBMS_Small*) is the dependent variable, the coefficients on *RW* and *AF* are positive and significant ($\beta_1 = 0.055$, t = 13.84, $p < 0.01$; $\beta_2 = 0.016$, t = 3.81, $p < 0.01$), but the coefficient on AF is much smaller ($p < 0.01$) than that on *RW*. Thus, small traders react more to the RW earnings surprises than to the AF earnings surprises.

When the large traders' excess net-buy (*EXBMS_Large*) is the dependent variable, the coefficient on *RW* is insignificant ($\beta_1 = 0.005$, t = 0.55, p = 0.58) and the coefficient on *AF* is positive and significant ($\beta_2 = 0.106$, t = 11.41, $p < 0.01$). Thus, large traders appear to respond only to analyst-based earnings surprises during the announcement period, consistent with Battalio and Mendenhall's (2005) result.

Testing H1 and H2: Trading during the Post-Earnings-Announcement Period

Panel B of Table 3 provides the results of estimating Equation (5) for the 60-day (i.e., [+6, +65]) period *after* the earnings announcements. When the cumulative abnormal return, *DRIFT* (i.e., the post-earnings-announcement drift), is the dependent variable, the coefficients on *RW* and *AF* are positive and significant ($\beta_1 = 0.040$, $t = 6.73$, $p < 0.01$; $\beta_2 = 0.043$, $t = 7.47$, $p < 0.01$), consistent with the existence of distinct RW and AF drifts (Livnat and Mendenhall 2006). The coefficients for *RW* and *AF* are not statistically different ($p = 0.71$).

Panel B of Table 3 also presents tests of H1 and H2. Consistent with H1, when we use the post-earnings-announcement small trader excess net-buy as the dependent variable, the coefficient on *RW* is positive and significant ($\beta_1 = 0.030$, $t = 15.21$, $p < 0.01$). This evidence indicates that small traders continue to trade in the direction of RW earnings surprises after earnings announcements. Consistent with H2, when the post-earnings-announcement large trader excess net-buy is the dependent variable, the coefficient on *AF* is positive and significant ($\beta_2 = 0.015$, $t = 4.30$, $p < 0.01$). This result indicates that large traders continue to trade in the direction of analyst-based earnings surprises after earnings announcements. In combination, this evidence is consistent with distinct sets of traders explaining, at least in part, the two post-earnings-announcement drifts.

Results in Table 3, Panel B also indicate a negative and significant coefficient on *AF* (-0.005, $t = -2.58$, $p = 0.01$) in the small trader net-buy regression, suggesting that, while small traders continue to trade in the direction of the RW earnings surprises, they also trade in the opposite direction of the AF earnings surprises. Similarly, we find a negative and significant coefficient on *RW* (-0.018, $t = -5.77$, $p < 0.01$) in the large trader net-buy regression, suggesting that, while large traders continue to trade in the direction of the AF earnings surprises after the earnings announcements, they also trade in the opposite direction of the RW earnings surprises. These results are similar to the relations we find in univariate comparisons.

A plausible explanation for large traders trading in the opposite direction of RW earnings surprises during the post-announcement period is that they provide liquidity to small traders trading on RW earnings surprises. Specifically, because large traders have an earnings expectation model different from that of small traders and consequently a different and independent estimate of the intrinsic value of the same firm, they may sell a security when the demand by small traders for the security pushes its price above large traders' private valuation of the security.

Liquidity trading is less likely to explain the negative association between trades by less sophisticated small traders and AF earnings surprises and, thus, we investigate an alternative explanation: attention-based trading. Lee (1992), Battalio and Mendenhall (2005), and Hirshleifer et al. (2008) find that small traders are net buyers irrespective of the direction of the earnings surprise. Barber and Odean (2008) argue that retail investors are heavily influenced by a "news attention" effect that prompts buying whenever a company is in the news. To test the possibility that the negative coefficient on *AF* may be due primarily to net-buying by small traders when *AF* is negative, we add to Equation (5) an indicator variable, *NegAF*, which equals 1 if the analyst-based earnings surprise is negative, and 0 otherwise. Consistent with individual net-buys following negative AF surprises explaining the negative association between small trades and AF surprises, the coefficient on *NegAF* is positive and significant, while the coefficient on *AF* becomes insignificant.[23] Further, following Barber and Odean (2008), we add trading volume, *Vol*, to Equation (5) to proxy for news attention and interact it with *NegAF*. We define *Vol* as the average daily trading volume during the earnings-announcement period $[-1, +1]$, scaled by the average daily trading volume during the control period $[-45, -6]$. Consistent with attention-based trading, the coefficient on *NegAF* × *Vol* is positive and significant while the coefficients on *NegAF* and *AF* are insignificant.[24]

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[23] To assure that attention trading does not explain the negative coefficient for *RW* in the large trade regression, we run a similar analysis for large trades, including an indicator variable, *NegRW*, which equals 1 if the seasonal random-walk earnings surprise is negative, and 0 otherwise. Including *NegRW* does not affect the negative coefficient for *RW*.

[24] The coefficient on *Vol* is also positive and significant. The positive and significant coefficients on *Vol* and *NegAF* × *Vol*

Testing H3 and H4: Effects of Announcement Period Trading Intensity on the Post-Earnings-Announcement Drift

H3 (H4) predicts that the intensity of earnings-announcement period trading by small (large) traders on RW (AF) earnings surprises reduces the magnitude of the post-earnings-announcement drift associated with RW (AF) earnings surprises. We run the following regression to test H3 and H4:

$$\begin{aligned} DRIFT_{it} = {} & \theta_0 + \theta_1 RW_{it} + \theta_2 AF_{it} + \theta_3 EXBMS\_Small_{it} + \theta_4 EXBMS\_Large_{it} \\ & + \theta_5 RW_{it} \cdot EXBMS\_Small_{it} + \theta_6 AF_{it} \cdot EXBMS\_Small_{it} + \theta_7 RW_{it} \cdot EXBMS\_Large_{it} \\ & + \theta_8 AF_{it} \cdot EXBMS\_Large_{it} + \theta_9 RW_{it} \cdot TransCost_{it} + \theta_{10} AF_{it} \cdot TransCost_{it} \\ & + \theta_{11} TransCost_{it} + \varepsilon_{2it}, \end{aligned} \quad (6)$$

where *DRIFT* is the 60-day [+6, +65] cumulative abnormal return. *EXBMS_Small* and *EXBMS_Large* are the three-day [−1, +1] announcement period excess net-buy for small and large traders, respectively. *TransCost* is a measure of transaction cost that takes on values between 0 and −1 and is defined as −1 times the average of scores on the following three dimensions: (1) the decile ranking (scaled to between 0 and 1) of market value at the end of the earnings announcement quarter, (2) the decile ranking (scaled to between 0 and 1) of trading volume over the preceding fiscal year ending in the earnings announcement quarter, and (3) an indicator variable that equals 1 if price at the end of the earnings announcement quarter is greater than $10, and 0 otherwise (Bhushan 1994; Kimbrough 2005).[25]

We test whether small (large) trades during the announcement period attenuate the relation between RW (AF) earnings surprises and post-announcement returns by interacting small (large) trades with RW (AF) earnings surprises in Equation (6). In this regression, the coefficient on the interaction term between small trades and RW earnings surprise (*RW·EXBMS_Small*) captures whether earnings announcement trading by small traders attenuates the association between RW earnings surprises and post-announcement returns (i.e., the RW drift), as posited by H3. The coefficient on the main effect of announcement period small trades (*EXBMS_Small*) captures the overall relation between small trades and post-announcement returns, unconditioned on RW earnings surprises. We expect a negative coefficient on *RW·EXBMS_Small.*

Likewise, the coefficient on the interaction term between large trades and AF earnings surprises (*AF·EXBMS_Large*) captures whether earnings announcement trading by large traders attenuates the association between the AF earnings surprises and post-announcement returns (i.e., the AF drift), as posited by H4. Similarly, the coefficient on the main effect of large trades (*EXBMS_Large*) captures the overall relation between announcement period large trades and post-announcement returns, unconditioned on AF earnings surprises. We expect a negative coefficient on *AF·EXBMS_Large.*

Transaction costs reduce the incentives for arbitrage activities and, *ceteris paribus*, should enhance the post-earnings-announcement drift. Accordingly, we expect the coefficients on *RW·TransCost* and *AF·TransCost* to be positive to the extent that transaction costs limit arbitrage opportunities associated with the RW and AF drifts.

Table 4 presents the results from estimating Equation (6). In Column (1), we report the results from estimating Equation (6) separately for small traders (i.e., excluding the $EXBMS\_Large_{it}$

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imply asymmetric net buying by small traders in response to the news effect, with small traders reacting more to negative AF news. This intriguing finding is consistent with negative news drawing more attention-based trading by small traders and warrants further study by future research.

[25] Results are similar if we use bid-ask spread as an alternative measure of transaction cost.

TABLE 4

Effects of Excess Net-Buy during Earnings Announcements Period on Post-Earnings-Announcement Drift

	(1)	(2)	(3)
Intercept	−0.066	−0.067	−0.066
	(−4.08)***	(−4.14)***	(−4.07)***
RW	0.097	0.097	0.097
	(4.07)***	(4.06)***	(4.07)***
AF	0.075	0.077	0.075
	(3.13)***	(3.23)***	(3.13)***
RW·EXBMS_Small	−0.044	—	−0.046
	(−3.20)***		(−3.29)***
AF·EXBMS_Small	0.009	—	0.011
	(0.47)		(0.56)
RW·EXBMS_Large	—	0.015	0.016
		(1.72)*	(1.82)*
AF·EXBMS_Large	—	−0.017	−0.018
		(−2.07)**	(−2.16)**
RW·TransCost	0.076	0.077	0.076
	(2.67)***	(2.71)***	(2.68)***
AF·TransCost	0.042	0.044	0.042
	(1.49)	(1.54)	(1.47)
EXBMS_Small	0.042	—	0.042
	(3.60)***		(3.61)***
EXBMS_Large	—	0.001	0.001
		(0.24)	(0.10)
TransCost	−0.027	−0.029	−0.027
	(−1.42)	(−1.54)	(−1.41)
R^2 (%)	0.36	0.33	0.37
Obs.	73,469	73,469	73,469

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*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.

All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

Table 4 shows regression of cumulative abnormal return during [+6, +65] window following earnings announcements. Abnormal return during the post-earnings-announcement period is defined as the firm return in excess of the corresponding Fama-French size and book-to-market six-portfolio benchmark return. *RW* is the decile rank of seasonal random-walk earnings surprise converted to [0, 1], and *AF* is the decile rank of analyst-based earnings surprise converted to [0, 1]. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*−4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*. *EXBMS_Small* is the average excess net-buy for small trades during [−1, +1] window around earnings announcements. Small trade is defined as a trade at maximum $5,000. *EXBMS_Large* is the average excess net-buy for large trades during [-1, +1] window around earnings announcements. Large trade is defined as a trade at minimum $30,000. *TransCost* is transaction cost taking on values between 0 and −1, and defined as −1 times the average of score on the following three dimensions: (1) the decile ranking of market value at the end of earnings announcement quarter, (2) the decile ranking of trading volume over preceding fiscal year ending in earnings announcement quarter, and (3) an indicator variable equals 1 if price at the end of earnings announcement quarter is greater than $10, and 0 otherwise.

variable and related interactions). In Column (2), we report the results from estimating Equation (6) separately for large traders (i.e., excluding the $EXBMS\_Small_{it}$ variable and related interactions), and in Column (3), we report the results including both the small trade and large trade variables.

In Column (1), the coefficients on *RW* and *AF* are positive and significant, verifying that the drift is associated with both RW and AF earnings surprises. In addition, consistent with H3, the coefficient on *RW·EXBMS_Small* is negative and significant ($\theta_5 = -0.044$, $t = -3.20$, $p < 0.01$). As predicted, this evidence suggests that small traders' announcement period trading intensity, conditioned on RW earnings surprises, reduces the magnitude of the RW drift.[26] In contrast, the coefficient on *AF·EXBMS_Small* is insignificant ($\theta_6 = 0.009$, $t = 0.47$, $p = 0.64$), indicating that small traders' announcement period trading intensity does not reduce the AF drift.[27] Finally, consistent with the drifts increasing with higher transaction costs, we find positive and significant coefficient on *RW·TransCost* (0.076, $t = 2.67$, $p < 0.01$) and positive but insignificant coefficient on *AF·TransCost* (0.042, $t = 1.49$, $p = 0.14$).[28] Consistent with H4, Column (2) reports a negative and significant coefficient on *AF·EXBMS_Large* ($\theta_8 = -0.017$, $t = -2.07$, $p = 0.04$). Thus, as predicted, large traders' announcement period trading intensity in the direction of AF earnings surprises reduces the AF drift. Interestingly, the coefficient on *RW·EXBMS_Large* is positive and marginally significant ($\theta_7 = 0.015$, $t = 1.72$, $p = 0.09$), suggesting that when large traders trade more intensely during the earnings-announcement period in the direction of RW earnings surprises, the RW drift is enhanced.[29] As in Column (1), Column (2) also reports positive coefficients on *RW·TransCost* (0.077, $t = 2.71$, $p < 0.01$) and *AF·TransCost* (0.044, $t = 1.54$, $p = 0.12$).

When we run regression Equation (6) with small and large traders together, we obtain similar results. Specifically, in Column (3) the coefficients on *RW·EXBMS_Small* and *AF·EXBMS_Large* are negative and significant ($\theta_5 = -0.046$, $t = -3.29$, $p < 0.01$; $\theta_8 = -0.018$, $t = -2.16$, $p = 0.03$). In sum, this analysis suggests that the RW and AF drifts, at least in part, are attributable to the under-reaction (i.e., incomplete trading) by different groups of investors with alternative earnings expectations models.

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[26] Hirshleifer et al. (2008) find that adding the *main* effect of individual investor trades during the announcement period to a regression of post-announcement returns on the RW earnings surprises does not decrease the power of RW to explain post-announcement returns. We are able to reconcile this result. Similar to Hirshleifer et al. (2008), we find that the *main* effect for net small trades does not decrease the power of RW earnings surprises to explain post-announcement returns. In contrast to Hirshleifer et al. (2008), we posit that small trades during the announcement period attenuates the RW portion of the drift (i.e., the association between the RW earnings surprise and post-announcement returns). To capture the statistical significance of this attenuation, we therefore *interact* small trades with the RW earnings surprises (and also include small trades as a main effect) in the regression of post-announcement returns on the RW earnings surprises (see Equation (6)).

[27] If small traders were to arbitrage the AF drift, then we would expect their trading intensity at the earnings announcements should be positively associated with their expected profits of this strategy (i.e., the magnitude of the AF drift), which would result in a positive relation between AF drift and the interaction term of *AF* and *EXBMS_Small*. The insignificant coefficient on the interaction term *AF* and *EXBMS_Small* in Table 4 is inconsistent with arbitrage by small traders.

[28] Evidence that the RW drift increases with transaction cost is consistent with transaction costs providing one explanation for why large traders do not completely eliminate the RW drift.

[29] Ke and Ramalingegowda (2005) and Ali et al. (2007) find evidence consistent with certain institutional investors exploiting the RW drift. A significant positive coefficient on *RW·EXBMS_Large* is consistent with large traders initiating trades around earnings announcements to profit from the RW drift. Specifically, in anticipation of the RW drifts, some sophisticated large traders may trade at the earnings announcements based on the RW surprises. Subsequently, they can trade against the orders of small traders for a profit during the post-earnings-announcement period. The trading intensity of large traders at the earnings announcements should be positively associated with their expected profits of this strategy (i.e., the magnitude of the RW drift), which would result in a positive relation between RW drift and the interaction term of *RW* and *EXBMS_Large*. We note, however, that based on the results in Panel A, Table 3, during the announcement period large traders *on average* do not trade on the RW earnings surprises (the coefficient on *RW* is insignificant). Therefore, as one might expect, arbitrage trading on the RW signal does not seem to be a significant motive for the *average* large trader at earnings announcements.

VI. EVIDENCE OF ADDITIONAL DIFFERENCES BETWEEN THE RW AND AF DRIFTS

We conduct several analyses to further explore and contrast the RW and AF drifts. While our evidence suggests that the RW and AF drifts are attributable to distinct investor groups that under-react to RW and AF earnings surprises, we expect the nature of these two drifts to be qualitatively different because large traders are considered relatively more sophisticated than small traders (e.g., as evidenced by their more sophisticated earnings expectation model). In this section we provide additional analyses that investigate the persistence of trading associated with each drift, the predictability of post-announcement trades based on prior earnings surprises, the roles of analyst forecast heterogeneity and analyst forecast revisions after earnings announcements in explaining the drifts, and how the RW and AF drifts have changed in recent years.

Persistence of Trading after the Earnings Announcements

While we find that small and large investors under-react to distinct forms of earnings surprises, we further investigate whether the persistence of investors' post-announcement trading varies predictably with investor sophistication. To the extent that small traders are relatively less sophisticated than large traders as prior studies argue (Bhattacharya 2001; Bhattacharya et al. 2007; Malmendier and Shanthikumar 2007), we expect small traders' trading on information in RW earnings surprises to continue longer during the post-earnings-announcement period and large traders' trading on information in AF earnings surprises to concentrate at the early stage of the post-announcement period.

To test our expectation, we divide the 60-day post-earnings-announcement period into three 20-day periods. We run the same regression Equation (5) for small and large traders during these three 20-day sub-periods. We expect that large traders' activities are more concentrated in the first 20-day sub-period, while small traders' activities are more evenly distributed among these three sub-periods.

Consistent with expectations, results in Panel A of Table 5 show that small traders' activities do not mainly concentrate in the early sub-period. In particular, the coefficient on *RW* is positive and significant in all three 20-day periods ($\beta_1 = 0.023$, $t = 9.59$, $p < 0.01$ for the first 20-day period, $\beta_1 = 0.033$, $t = 12.80$, $p < 0.01$ for the second 20-day period, and $\beta_1 = 0.034$, $t = 11.70$, $p < 0.01$ for the third 20-day period). In fact, it appears that small traders' trading activities are persistent and even become more intensified over time. Indeed, using a stacked regression including each 20-day period, we find that the coefficients for the second and third 20-day periods are significantly larger than the coefficient for the first 20-day period ($t = 4.36$, $p < 0.01$ and $t = 3.78$, $p < 0.01$, respectively).[30] Panel B reports results for large traders. The coefficients on *AF* are positive and decline in magnitude ($\beta_2 = 0.018$, $t = 3.27$, $p < 0.01$ for the first 20-day period, $\beta_2 = 0.014$, $t = 2.46$, $p = 0.01$ for the second 20-day period, and $\beta_2 = 0.010$, $t = 2.05$, $p = 0.04$

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[30] We conjecture that feedback-loop trading (De Long et al. 1990a; 1990b) and/or increased attention (Barber and Odean 2008) cause small traders' trading intensity to increase in subperiods after the earnings announcements. Feedback-loop trading suggests that a subset of small traders trade in an early subperiod and the price changes due to their trading cause another subset of small traders to trade in a later subperiod. Increased attention suggests that a subset of small traders trade more when the stock catches more of their attention as other investors trade more. To determine which conjecture more likely explains the increased small traders' trading intensity after earnings announcements, we regress small traders' trading during the third subperiod [+46, +65] on the two forms of earnings surprises, *RW* and *AF*, the cumulative abnormal return during [+6, +45] $CAR_{+6,+45}$ (or trading volume during [+6, +45] $Vol_{+6,+45}$) and its interaction with the random-walk-based earnings surprise $RW \times CAR_{+6,+45}$ (or $RW \times Vol_{+6,+45}$). The coefficients on $RW \times CAR_{+6,+45}$ and $RW \times Vol_{+6,+45}$ are positive and significant, supporting both the feedback-loop trading conjecture and the increased attention conjecture. However, when $RW \times CAR_{+6,+45}$ and $RW \times Vol_{+6,+45}$ are included simultaneously in the model, the effect of $RW \times CAR_{+6,+45}$ becomes insignificant while the effect of $RW \times Vol_{+6,+45}$ remains positive and significant, suggesting that the attention-based trading effect is likely more dominant.

TABLE 5

Sub-Period Regressions of Excess Net-Buy for Small and Large Trades

Panel A: Small Trades

	[+6, + 25]	[+26, +45]	[+46, +65]
Intercept	0.011	−0.003	0.004
	(7.05) ***	(−1.74) **	(2.44)***
RW	0.023	0.033	0.034
	(9.59) ***	(12.80) ***	(11.70)***
AF	−0.010	−0.007	−0.000
	(−3.91) ***	(−2.37) **	(−0.11)
R^2 (%)	0.11	0.20	0.20
Obs.	73,469	73,469	73,469

Panel B: Large Trades

	[+6, +25]	[+26, +45]	[+46, +65]
Intercept	0.005	0.013	0.018
	(1.39)	(4.13) ***	(5.09)***
RW	−0.013	−0.020	−0.022
	(−2.47) **	(−4.17) ***	(−3.98)***
AF	0.018	0.014	0.010
	(3.27) ***	(2.46) **	(2.05)**
R^2 (%)	0.02	0.03	0.03
Obs.	73,469	73,469	73,469

*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.

All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

Panel A and Panel B of Table 5 shows regression of average excess net-buy of small trades and average net-buy of large trades during three sub-periods following earnings announcements, [+6, +25], [+26, +45], and [+46, +65]. *RW* is the decile rank of seasonal random-walk earnings surprise converted to [0, 1], and *AF* is the decile rank of analyst-based earnings surprise converted to [0, 1]. Seasonal random walk-earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*–4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*. Small trade is defined as a trade at maximum $5,000. Large trade is defined as a trade at minimum $30,000.

for the third 20-day period). Using a stacked regression including each 20-day period, we find that the coefficients for the second and third 20-day periods are significantly smaller than the coefficient for the first 20-day period (t = −1.67, p = 0.09 and t = −1.83, p = 0.07, respectively). Thus, consistent with our expectation, large traders' trades are more concentrated during the early stage of the post-announcement period.[31]

Trading around Earnings Announcements and Prior Earnings Surprises

If small traders fixate on RW earnings surprises and are unaware of the serial correlations of RW earnings surprises, then we expect that small trades around earnings announcements are associated with lagged RW earnings surprises (Bernard and Thomas 1990). In contrast, if large traders are more sophisticated and their under-reaction is relatively short-lived, then their trades around earnings announcements are less likely to be associated with lagged AF earnings surprises. To examine the associations of trades at earnings announcements and lagged earnings surprises, we run the following regression:

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[31] We find that the association between large trades and AF surprises is no longer significant (p = 0.18) in the [+46, +65] period when we use a stronger cut on outliers (top and bottom 2 percent). Hence, it appears that the significant association between large trades and AF surprises over the [+46, +65] period is driven by a small number of outliers.

$$EXBMS\_Small_{it}/EXBMS\_Large_{it} = \delta_0 + \delta_1 RW_{it-q} + \delta_2 AF_{it-q} + \varepsilon_{4it}, \quad q = 1,2,3, \text{ or } 4, \tag{7}$$

where q represents lagged one to four quarters.[32]

Table 6 reports the results from estimating Equation (7). We find that average excess net-buy for small traders around earnings announcements are positively associated with RW_{t-1} (0.043, $t = 9.33$, $p < 0.01$), RW_{t-2} (0.027, $t = 5.84$, $p < 0.01$), RW_{t-3} (0.024, $t = 5.21$, $p < 0.01$), and RW_{t-4} (0.017, $t = 3.78$, $p < 0.01$). These results suggest that small trades around one- to four-quarter-ahead earnings announcements are predictable based on prior RW earnings surprises. On the other hand, we find that excess net-buys for large traders are not associated with prior AF earnings surprises, indicating that large trades around future earnings announcements cannot be predicted based on prior AF earnings surprises. This evidence suggests that the RW drift (but not AF drift) is largely explained by small or unsophisticated traders' failure to recognize the time-series properties of earnings, consistent with investor naiveté.

Heterogeneous Forecasts and Post-Announcement Trading

To further differentiate the RW and AF drifts, we investigate whether the AF drift and post-announcement trades by large traders associated with the AF earnings surprise are more pronounced when the AF earnings surprise is more difficult to interpret (i.e., a longer price discovery process when earnings surprises are more difficult to interpret). Prior research finds that the AF drift is greater when analysts' forecasts are more heterogeneous (Liang 2003), suggesting that investors have difficulty interpreting AF earnings surprises when analysts' forecasts are less correlated. Based on Liang (2003), we estimate the following regression model to test the effects of forecast heterogeneity on drift and post-announcement trading:

$$\begin{aligned} CAR_{it}/EXBMS\_Large_{it}/EXBMS\_Small_{it} = {} & \gamma_0 + \gamma_1 RW_{it} + \gamma_2 AF_{it} + \gamma_3 AF_{it} \cdot Hetero_{it} \\ & + \gamma_4 Hetero_{it} + \varepsilon_{6it} \end{aligned} \tag{8}$$

where $Hetero$ equals 1 minus the BKLS analyst consensus (i.e., Barron et al. 1998). Specifically, we compute $Hetero$ as:

$$1 - \frac{SE - D/N}{(SE - D/N) + D} \tag{9}$$

where D is the forecast dispersion, the sample variance of one-year-ahead annual earnings forecasts reported within 30 days following the earnings announcement; N is the number of those forecasts; and SE is the squared difference between the actual annual EPS and the mean of those

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[32] We start with a replication of the serial correlations of RW and AF earnings surprises (Bernard and Thomas 1990; Abarbanell and Bernard 1992) for our sample. Consistent with Bernard and Thomas (1990), we find a "+, +, +,−" pattern for first- to fourth-order serial correlations among RW earnings surprises with declining magnitudes (replication results not tabulated). Consistent with Abarbanell and Bernard (1992), we find positive serial correlations among AF earnings surprises for first- to fourth-order serial correlations with declining magnitudes (replication results not tabulated). We also replicate prior results that future returns around earnings announcements are predictable by regressing three-day *CAR* regressions on lagged RW earnings surprises. We find that three-day *CAR* is positively associated with lagged one and two quarters' RW earnings surprises and lagged one to three AF earnings surprises. Further, the coefficients on RW and AF earnings surprises decline in magnitude from lagged one to lagged four quarters (replication results not tabulated).

TABLE 6

Excess Net-Buy for Small and Large Trades and Lagged Earnings Surprises

	Small Trades				Large Trades			
Intercept	0.023 (8.27) ***	0.029 (10.60) ***	0.032 (10.96) ***	0.037 (12.83)***	0.032 (5.23)	0.037 (5.92)	0.035 (5.37)	0.036 (5.85)
RW_{t-1}	0.043 (9.33) ***	—	—	—	−0.018 (−1.84) *	—	—	—
RW_{t-2}	—	0.027 (5.84) ***	—	—	—	−0.016 (−1.63)	—	—
RW_{t-3}	—	—	0.024 (5.21) ***	—	—	—	−0.014 (−1.44)	—
RW_{t-4}	—	—	—	0.017 (3.78)***	—	—	—	−0.010 (−1.01)
AF_{t-1}	0.004 (1.05)	—	—	—	0.016 (1.61)	—	—	—
AF_{t-2}	—	0.008 (1.28)	—	—	—	0.002 (0.16)	—	—
AF_{t-3}	—	—	0.005 (1.07)	—	—	—	0.007 (0.73)	—
AF_{t-4}	—	—	—	0.001 (0.34)	—	—	—	−0.001 (−0.15)
R^2 (%)	0.18	0.08	0.06	0.03	0.01	0.00	0.00	0.00
n	63,223	62,526	61,896	61,665	63,223	62,526	61,896	61,665

*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.

All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

Table 6 shows regressions of average excess net-buy of small and large trades during [−1, +1] earnings announcement window on lag earnings surprises. RW_{t-q} ($q = 1, 2, 3$ or 4) is the decile rank of seasonal random-walk earnings surprise for quarter $t-q$ converted to [0, 1]. AF_{t-q} ($q = 1, 2, 3$ or 4) is the decile rank of analyst-based earnings surprise for quarter $t-q$ converted to [0, 1]. Seasonal random-walk-earnings surprise is defined as I/B/E/S actual EPS for quarter t minus I/B/E/S actual EPS for quarter $t-4$, deflated by price per share at the beginning of quarter t. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter t minus most recent forecast for quarter t, deflated by price per share at the beginning of quarter t. Small trade is defined as a trade at maximum $5,000. Large trade is defined as a trade at minimum $30,000.

forecasts. If there is only one forecast available, then we set *Hetero* to 0 (i.e., no heterogeneous information contained in analysts' forecasts). Consistent with Liang (2003), we transform *Hetero* into deciles scaled between [0, 1]. We predict that the coefficient for *AF·Hetero* should be positive in both the *CAR* and *EXBMS_Large* regressions if the AF drift and post-announcement trading on AF earnings surprises by large traders is more pronounced when analysts' forecasts are more heterogeneous.

Results in Table 7 are consistent with our expectation. Specifically, we find that the coefficient on *AF·Hetero* is positive and significant in the large traders' trading regression ($\gamma_3 = 0.009$, t = 2.67, p = 0.01), indicating that large traders' post-earnings-announcement period trading associated with AF earnings surprises is more pronounced when analysts' forecasts are more heterogeneous. Similar to Liang (2003), we also find a positive coefficient on *AF·Hetero* in the *CAR*

TABLE 7

Relation between Heterogeneity of Analysts' Forecasts and Returns and Excess Net-Buy during the Post-Earnings-Announcement Period

	60-Day Post-Announcement Period		
	DRIFT	**Small Trades**	**Large Trades**
Intercept	−0.047	0.005	0.005
	(−12.68)***	(3.67)***	(3.67)***
RW	0.041	0.030	−0.025
	(6.62)***	(14.26)***	(−6.96)***
AF	0.012	−0.011	0.006
	(1.51)	(−3.41)***	(1.90)*
AF·Hetero	0.056	−0.030	0.009
	(5.83)***	(−8.02)***	(2.67)***
Hetero	0.016	−0.002	0.001
	(14.94)***	(−4.18)***	(2.02)**
R^2 (%)	0.32	0.39	0.37
Obs.	66,726	66,729	66,726

*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.

All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

Table 7 shows regressions of cumulative abnormal return, average excess net-buy of small trades, and excess average net-buy of large trades during [+5, +65] window following earnings announcements. Abnormal return during the post-earnings-announcement period is defined as the firm return in excess of the corresponding Fama-French size and book-to-market six-portfolio benchmark return. *RW* is the decile rank of seasonal random-walk earnings surprise converted to [0, 1], and *AF* is the decile rank of analyst-based earnings surprise converted to [0, 1]. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*–4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*. Following Liang (2003), *Hetero* equals 1 minus the BKLS analyst consensus (i.e., Barron et al. 1998). Specifically, we compute *Hetero* as:

$$1 - \frac{SE - D/N}{(SE - D/N) + D}$$

where *D* is the forecast dispersion, the sample variance of one-year-ahead annual earnings forecasts reported within 30 days following earnings announcement; *N* is the number of those forecasts; *SE* is the squared difference between the actual annual EPS and the mean of those forecasts. If there is only one forecast available, *Hetero* is defined as 0 (i.e., no heterogeneous information contained in analysts' forecasts). Consistent with Liang (2003), *Hetero* is transformed into deciles and converted to [0, 1]. Small trade is defined as a trade at maximum $5,000. Large trade is defined as a trade at minimum $30,000.

regression ($\gamma_3 = 0.056$, $t = 5.83$, $p < 0.01$). This evidence is consistent with investors and analysts requiring more post-announcement information or price discovery to interpret earnings surprises when analyst forecasts are more heterogeneous. Finally, consistent with the pattern in our previous analyses, the results for small traders are opposite to the results for large traders. Specifically, we find a negative and significant coefficient for *AF·Hetero* in the small trader regression ($\gamma_3 = -0.030$, $t = -8.02$, $p < 0.01$).

Effect of Analyst Forecast Revisions during the Post-Announcement Period

We next investigate how analyst forecast revisions in the post-earnings-announcement period affect large and small traders' post-announcement trading on earnings surprises. We propose that large traders' under-reaction is corrected in the post-announcement period through a price discovery process and offer analyst forecast revisions as one source of information discovery (Gleason and Lee 2003). Specifically, we conjecture that post-announcement trades on AF earnings surprises may be at least partially explained by analyst forecast revisions in the post-announcement period that are correlated with AF earnings surprises. Because our previous results suggest that small traders are less likely to trade on AF earnings surprises, we expect that analyst forecast revisions are less likely to explain small traders' post-announcement trades. We estimate the following regression to determine whether large and small trades on RW and AF earnings surprises in the post-announcement period are partially explained by post-announcement analyst forecast revisions:

$$DRIFT_{it}/EXBMS\_Small_{it}/EXBMS\_Large_{it} = \lambda_0 + \lambda_1 RW_{it} + \lambda_2 AF_{it} + \lambda_3 Rev_{it} + \varepsilon_{5it}, \quad (10)$$

where *Rev* is the decile rank of analyst forecast revisions during the post-earnings-announcement period. We compute an analyst's forecast revision as an analyst's most recent forecast issued during [+46, +65] window minus the *same* analyst's earliest forecast issued during the [+6, +25] window, scaled by lag stock price (Gleason and Lee 2003). To aggregate analyst-level revisions to firm-level, we average individual analysts' revisions for each announcement event. We code forecast revisions as 0 if the I/B/E/S Detail file reports no associated forecasts.

We report regression results for Equation (10) in Panel A of Table 8.[33] In the regression with post-announcement stock returns as the dependent variable, the coefficient on *Rev*, is positive, 0.273, and significant ($t = 51.57$, $p < 0.01$). As expected, this evidence suggests that market returns incorporate information contained in analyst forecast revisions. Controlling for post-announcement revisions causes the coefficient on *RW* to drop from 0.040 ($t = 6.73$, $p < 0.01$) to 0.013 ($t = 2.30$, $p = 0.02$; the coefficient decrease is significant at $p < 0.01$) and the coefficient on *AF* to drop from 0.043 ($t = 7.47$, $p < 0.01$) to 0.020 ($t = 3.51$, $p < 0.01$; the coefficient decrease is significant at $p < 0.01$). This evidence suggests that the information captured by analyst forecast revisions explains substantial portions of the RW and AF drifts.

When we use excess buy-sell order imbalance of large traders as the dependent variable, the coefficient on *Rev* is positive, 0.027, and significant ($t = 12.93$, $p < 0.01$), consistent with large traders incorporating the information contained in analyst forecast revisions in their trades. With the inclusion of revisions, the coefficient on *AF* declines from 0.015 ($t = 4.30$, $p < 0.01$) to 0.008 ($t = 3.37$, $p < 0.01$), a 46.67 percent decrease (significant at $p < 0.01$), whereas the coefficient on *RW* remains largely unchanged. This evidence suggests that much of the post-announcement trading by large traders based on AF earnings surprises is attributable to information contained in

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[33] The Pearson correlation between *RW* and *Rev* is 0.1234 and between *AF* and *Rev* is 0.1147, consistent with prior research showing an association between post-earnings-announcement analyst forecast revisions in the post-announcement period and both random-walk earnings changes and analyst-based earnings surprises (Abarbanell and Bernard 1992).

TABLE 8

Effects of Analyst Forecast Revisions Following Earnings Announcements on Drifts and Trades

Panel A: 60-Day Post-Announcement Period

	DRIFT		Small Trade		Large Trade	
Intercept	−0.047	−0.113	0.004	0.006	0.012	0.009
	(−13.29)***	(−30.33)***	(3.46)***	(4.68)***	(5.84)***	(5.71)***
RW	0.040	0.013	0.030	0.031	−0.018	−0.020
	(6.73)***	(2.30)**	(15.21)***	(15.43)***	(−5.77)***	(−8.58)***
AF	0.043	0.020	−0.005	−0.005	0.015	0.008
	(7.47)***	(3.51)***	(−2.58)***	(−2.26)**	(4.30)***	(3.37)***
Rev		0.273		−0.008		0.027
		(51.57)***		(−4.02)***		(12.93)***
R^2 (%)	0.26	4.66	0.28	0.30	0.16	0.29
Obs.	73,469	73,469	73,469	73,469	73,469	73,469

Panel B: Large Trades in Three 20-Day Sub-Post-Announcement Periods

	[+6, +25]		[+26, +45]		[+46,+65]	
Intercept	0.005	0.002	0.013	0.006	0.018	0.005
	(1.39)	(0.60)	(4.13)***	(1.96)***	(5.09)***	(1.40)
RW	−0.013	−0.014	−0.020	−0.025	−0.022	−0.026
	(−2.47)**	(−2.63)***	(−4.17)***	(−4.96)***	(−3.98)***	(−4.90)***
AF	0.018	0.017	0.014	0.009	0.010	0.005
	(3.27)***	(3.14)***	(2.46)**	(1.72)*	(2.05)**	(1.46)
Rev		0.018		0.032		0.043
		(4.44)***		(7.50)***		(8.99)***
R^2 (%)	0.02	0.04	0.03	0.10	0.03	0.14
Obs.	73,469	73,469	73,469	73,469	73,469	73,469

*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.
All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

(continued on next page)

Panel A of Table 8 shows regression of cumulative abnormal return, average excess net-buy of small trades, and average excess net-buy of large trades during [+6, +65] window following earnings announcements. Abnormal return during the post-earnings-announcement period is defined as the firm return in excess of the corresponding Fama-French size and book-to-market six-portfolio benchmark return. Small trade is defined as a trade at maximum $5,000. Large trade is defined as a trade at minimum $30,000. *RW* is the decile rank of seasonal random-walk earnings surprise converted to [0, 1], and *AF* is the decile rank of analyst-based earnings surprise converted to [0, 1]. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter t minus I/B/E/S actual EPS for quarter t–4, deflated by price per share at the beginning of quarter t. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter t minus most recent forecast for quarter t, deflated by price per share at the beginning of quarter t. We compute an analyst's forecast revision as an analyst's most recent forecast issued during [+46, +60] window minus the same analyst's earliest forecast issued during the [+6, +25] window, scaled by lag stock price. To aggregate analyst-level revisions to firm-level, we average individual analysts' revisions for each announcement event. *Rev* is the decile rank (converted to [0, 1]) of this variable. Forecast revisions are coded as 0 if no associated forecasts are reported in the I/B/E/S Detail file.

Panel B of Table 8 shows regression of excess net-buy of large trades during [+6, +25], [+26, +45], and [+46, +65] subperiods. In Panel B, we compute an analyst's forecast revision as an analyst's most recent forecast issued during the respective 20-day subperiod minus the same analyst's previous most recent forecast, scaled by lag stock price. We then aggregate analyst-level revisions to firm-level by averaging individual analyst forecast revisions. *Rev* is the decile rank (converted to [0, 1]) of this variable.

analyst forecast revisions that continue to be correlated with AF earnings surprises well after the earnings announcements. This evidence is also consistent with the AF drift being largely attributable to a longer price discovery process by large traders, with analyst forecast revisions serving as one source of information discovery. In the small-trade regression, the coefficients on *RW* and *AF* are unchanged, while the coefficient on *Rev* is negative and significant. This evidence suggests that post-announcement small trades associated with RW earnings surprises are not explained by information contained in analyst forecasts.[34]

Panel B of Table 8 presents results of large trades in the three 20-day subperiods. In this analysis, we compute an analyst's forecast revision as an analyst's most recent forecast issued during the respective 20-day subperiod minus the same analyst's previous most recent forecast, scaled by lag stock price. We then aggregate analyst-level revisions to firm-level by averaging individual analyst forecast revisions and calculate *Rev* as the decile rank of the firm-level amounts. We find that the association between large trades and AF surprises becomes less significant during the [+26, +45] period and completely disappears during the [+46, +65] period after we control for *Rev*, whereas the association between large trades and *Rev* remains significant across each subperiod. This evidence is consistent with price discovery through analyst forecast revisions explaining the association between AF surprises and large trades well after earnings announcements.[35]

Market and Trading Reactions to Earnings Surprises Over Time

In recent years, analyst forecasts have become more readily available to all investors, which may result in small traders shifting away from RW earnings expectations (Brown and Caylor 2005). Likewise, in recent years, the analyst forecast drift appears to have received more attention, which could result in decreased AF drift as investors either arbitrage the drift or simply react more thoroughly to AF earnings surprises at earnings announcements. We explore these possibilities by introducing a time trend indicator into the following regression model:

$$DRIFT_{it}CAR_{it}/EXBMS\_Small_{it}/EXBMS\_Large_{it}$$
$$= \varphi_0 + \varphi_1 RW_{it} + \varphi_2 AF_{it} + \varphi_3 RW_{it} \cdot Recent_{it} + \varphi_4 AF_{it} \cdot Recent_{it} + \varphi_5 Recent_{it} + \varepsilon_{7it} \qquad (11)$$

where *Recent* is an indicator variable that equals 1 if an earnings announcement occurs after 1999, and 0 otherwise.[36]

Table 9 reports the results from this analysis. During the three-day earnings-announcement period, when we use abnormal return as the dependent variable, the coefficient on *RW·Recent* is insignificant, while the coefficient on *AF·Recent* is positive and significant (0.009, $t = 4.36$, $p < 0.01$), suggesting the market reacts more to AF earnings surprises in more recent years, consistent with Brown and Caylor (2005). When we use excess net-buy of small traders as the dependent variable, the coefficient on *RW·Recent* is negative and significant (-0.066, $t = -8.01$, $p < 0.01$)

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[34] We obtain similar results in tests in which we include three separate analyst forecast revision variables defined as the difference between the mean value of all annual forecasts issued during the second half of the trading month (e.g., the 1st, 2nd, or 3rd month following the earnings announcement) and the mean value of annual forecasts issued during the first half of the respective month.

[35] To provide further evidence that the AF drift may reflect a price discovery process through analyst forecast revisions after earnings announcements, we sort sample firms into quintile portfolios based on forecast revision frequency and examine how the AF drift varies across these portfolios in the post-announcement period. Consistent with a price discovery story, we find that when analysts' forecast revision frequency increases across Quintiles 2 through 5, the AF drift becomes more pronounced.

[36] We use the year 1999 to split the sample for two reasons. First, this split roughly divides our sample in half. Second, CUSUM statistics (Han and Park 1989) based on recursive least-squares regressions of the drift indicate a structural shift in 1999.

TABLE 9

Changes in the Market and Trade Reactions to Earnings Surprises over Time

	Three-Day Announcement Period			60-Day Post-Announcement Period		
	CAR	Small Trades	Large Trades	*DRIFT*	Small Trades	Large Trades
Intercept	−0.028	0.025	−0.061	−0.056	0.001	0.024
	(−27.92)***	(5.07)***	(−7.08)***	(−10.35)***	(0.36)	(8.38)***
RW	0.011	0.090	0.007	0.029	0.059	−0.036
	(6.85)***	(12.42)***	(0.54)	(3.27)***	(15.63)***	(−7.80)***
AF	0.051	−0.000	0.144	0.083	−0.017	0.019
	(31.08)***	(−0.05)	(10.64)**	(9.15)***	(−4.14)***	(1.78)*
RW ∗ Recent	0.002	−0.066	−0.005	0.018	−0.053	0.033
	(0.89)	(−8.01)***	(−0.28)	(1.30)	(−12.85)***	(5.28)***
AF ∗ Recent	0.009	0.027	−0.067	−0.073	0.020	−0.011
	(4.36)***	(3.06)***	(−3.63)***	(−6.11)***	(4.33)***	(−1.67)*
Recent	−0.006	−0.025	0.064	0.016	0.007	−0.003
	(−4.35)***	(−4.49)***	(5.57)***	(2.32)**	(2.61)***	(−5.22)***
R^2 (%)	5.82	0.98	0.34	0.35	0.58	0.12
n	73,469	73,469	73,469	73,469	73,469	73,469

*, **, *** Represent significance levels at less than two-tailed 10 percent, 5 percent, and 1 percent, respectively.

All t-statistics in parentheses are adjusted for clustering by firm (Rogers 1993).

Table 9 shows regression of cumulative abnormal return, average excess net-buy of small trades, and average excess net-buy of large trades during [−1, +1] earnings announcement window and during [+6, +65] window following earnings announcements. Abnormal return during post-earnings-announcement period is defined as firm return in excess of corresponding Fama-French size and book-to-market six-portfolio benchmark return. *RW* is the decile rank of seasonal random-walk earnings surprise converted to [0, 1], and *AF* is the decile rank of analyst-based earnings surprise converted to [0, 1]. Seasonal random-walk earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus I/B/E/S actual EPS for quarter *t*–4, deflated by price per share at the beginning of quarter *t*. Analyst-based earnings surprise is defined as I/B/E/S actual EPS for quarter *t* minus most recent forecast for quarter *t*, deflated by price per share at the beginning of quarter *t*. *Recent* is an indicator variable that equals 1 if an earnings announcement occurs after 1999, and 0 otherwise. Small trade is defined as a trade at maximum $5,000. Large trade is defined as a trade at minimum $30,000.

while the coefficient on *AF·Recent* is positive and significant (0.027, $t = 3.06$, $p < 0.01$), suggesting that small traders decrease their reliance on RW earnings expectations and increase their use of analyst forecasts in more recent years (e.g., as analyst forecasts are more accessible). In comparing the sum of the coefficients for *RW* and *RW·Recent* (0.090 + −0.066 = 0.024) to the sum of the coefficients for *AF* and *AF·Recent* (−0.000 + 0.027 = 0.027), evidence suggest that in the later part of our sample period small traders trade approximately equally on RW and AF earnings surprises ($F = 0.09$, $p = 0.763$). In contrast, large traders continue to trade only on AF earnings surprises, although the association between large trades and the AF earning surprise declines in the later part of the sample period (i.e., the coefficient for *AF·Recent* is negative and significant (−0.067, $t = -3.63$, $p < 0.01$)).

During the post-earnings-announcement period, the magnitude of the AF drift decreases in more recent years (i.e., the coefficient for *AF·Recent* is negative and significant (−0.073, $t = -6.11$, $p < 0.01$) while the RW drift does not change (i.e., the coefficient for *RW·Recent* is insignificant). This evidence is consistent with a more efficient processing of information contained in the AF earnings surprises in more recent years as the AF drift has received more attention.[37] Given the relatively new discovery and the little attention that the AF drift had received during the earlier years in our sample, we speculate that sophisticated investors may have not been exploiting the AF drift during this time period, which could explain the larger AF drift in earlier years.

When we use excess net-buy of small traders as the dependent variable, we find that small traders' post-announcement trading intensity related to *RW* decreases in more recent years (the coefficient for *RW·Recent* is -0.053, $t = -12.85$, $p < 0.01$), while that related to *AF* increases in more recent years (coefficient for *AF·Recent* is 0.020, $t = 4.33$, $p < 0.01$). These results are consistent with the trading results around earnings announcements (i.e., decreased trading by small traders on RW earnings surprises).[38] However, we continue to find a significant association between small trades and RW earnings surprises in the post-announcement period in the more recent time period (i.e., the sum of *RW* and *RW·Recent* is 0.006, $F = 11.66$, $p < 0.01$).

When we use excess net-buy of large traders as the dependent variable, we find a decrease in trading on AF earnings surprises in the post-earnings-announcement period (i.e., *AF·Recent* is -0.011, $t = -1.67$, $p = 0.09$). Nonetheless, we continue to find a significant association between large trades and AF earnings surprises in the post-announcement period in the more recent time period (i.e., the sum of *AF* and *AF·Recent* is 0.008, $F = 2.98$, $p = 0.09$).

VII. SUMMARY AND CONCLUSION

Recent research finds two distinct post-earnings-announcement drifts associated with RW and AF earnings surprises. We hypothesize that these two drifts are attributable to the trading activities of small and large traders who under-react to different forms of earnings innovations. Consistent with our hypothesis, we find that small (large) traders continue to trade in the direction of RW (AF) earnings surprises after earnings announcements. We corroborate these findings with evidence that when small (large) traders react more thoroughly to RW- (AF-) based earnings surprises during the announcement period, the respective drift attenuates.

In additional tests, we predict and find that the timing of post-announcement trading varies with trade size—i.e., large (and *relatively* more sophisticated) traders end their post-announcement trading more quickly than small (and *relatively* less sophisticated) traders, and that small trades (but not large trades) at earnings announcements are predictable based on prior earnings surprises. Further, we find that large traders' post-announcement trading is more pronounced when AF earnings surprises are more difficult to interpret (i.e., when analysts' forecasts are more heterogeneous) and that large traders' post-announcement trades based on AF earnings surprises are largely explained by the information contained in post-announcement analyst forecast revisions that are generally in the same directions of the earnings surprises. Finally, we find that the AF drift and related large trades during the post-announcement period have decreased in more recent years as the AF drift may have received more attention.

This study makes the following contributions. First, we predict and find that small traders systematically trade in the direction of RW earnings surprises after earnings announcements, whereas large traders' trading during post-earnings-announcement period is in the direction of AF earnings surprises. This evidence suggests that the RW and AF drifts are attributable, at least in part, to distinct sets of investors who use different earnings expectation models. Second, we provide evidence that trading activities during the earnings-announcement period affect the post-earnings-announcement drift. To our knowledge, this is the first study to link either *announcement period trading* or *post-announcement period trading* by distinct groups of investors to the RW and AF *drift*. Third, we find that the two drifts are qualitatively different—delayed small trades associated with random-walk-based surprises are consistent with small traders' failure to understand time-series properties of earnings, whereas delayed large trades associated with analyst-based surprises are more consistent with a longer price discovery process. Finally, we find that the AF drift and related large trades during the post-announcement period have decreased in more recent years as the AF drift appears to have received more attention.

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[37] The sum of *AF* and *AF·Recent* is 0.010 ($F = 3.82$, $p = 0.08$), suggesting a marginally significant AF drift in more recent years (2000–2005). Though we lack TAQ data for 2006 and 2007, we examined the AF drift for these years and find no evidence of an AF drift, consistent with a declining AF drift in recent years.

[38] To determine if the small trade results are robust to a period when sophisticated traders less frequently split their trades into smaller trades, we re-estimated Equation (11) focusing on the pre-1999 subsample period and re-defining *Recent* equal to 1 if the sample year is between 1996 and 1998, and 0 if the sample year is between 1993 and 1995. Results are similar—e.g., we find positive coefficients for *AF·Recent* in the small trade regressions during the earnings-announcement and the post-earnings-announcement periods.

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Discretionary Disclosure in Financial Reporting: An Examination Comparing Internal Firm Data to Externally Reported Segment Data

Daniel A. Bens
The University of Arizona
Philip G. Berger
The University of Chicago
Steven J. Monahan
INSEAD

ABSTRACT: We use confidential, U.S. Census Bureau, plant-level data to investigate aggregation in external reporting. We compare firms' plant-level data to their published segment reports by grouping a firm's plants that share the same four-digit SIC code into a "pseudo-segment." We then determine whether each pseudo-segment is disclosed as an external segment, or whether it is subsumed into a different business unit for external reporting purposes. We show that a pseudo-segment is more likely to be aggregated when the agency and proprietary costs of separately reporting the pseudo-segment are higher and when firm and pseudo-segment characteristics allow for more

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discretion in the application of segment reporting rules. For firms reporting multiple external segments, aggregation of pseudo-segments is driven by both agency and proprietary costs. For firms reporting a single external segment, we find no evidence of an agency cost motive for aggregation.

Keywords: *manufacturing plants; micro-level data; segment reporting; discretionary disclosure; agency costs; proprietary costs.*

Data Availability: *All data are available from public sources, except for the Census micro-level data.*

I. INTRODUCTION

We use a Census Bureau database of *confidential*, plant-level data to investigate two questions: What factors motivate managers of publicly traded firms to aggregate segment information when making disclosure decisions? How do private competitors affect both the disclosure decisions and industry-level statistics used to construct explanatory variables?

The Census data give us three advantages over prior research. First, we better observe management's private information endowment. Past segment-disclosure research (e.g., Harris 1998; Botosan and Stanford 2005; Berger and Hann 2007) has examined managers' aggregation decisions by evaluating the amount of segment information in *publicly* disseminated financial statements. A limitation of this approach is that the researcher does not consider the underlying source data that management observe. Because the Census data contain confidential information about a firm's operations that are less aggregated than the information in public filings, we are able to: (1) observe internal activities conducted by the firm, even if these activities are not separately disclosed in the segment footnotes, and (2) more accurately measure variables.

Observing management's private information endowment is made more valuable by the linkage between the agency cost and proprietary cost hypotheses for nondisclosure that we investigate. Agency costs of segment disclosure may arise when segment data provide information about value-reducing aspects of a firm's diversification strategy. If agency costs were the only plausible motive for nondisclosure, then there would be no point in withholding information, as rational market participants would infer that nondisclosure signals the worst possible outcome (Grossman 1981; Milgrom 1981). Thus, a plausible proprietary cost motive is necessary for the agency cost motive to potentially exist. Moreover, the agency and proprietary cost motives for aggregation are reasonable only if outsiders cannot use *publicly available* disclosures to fully unravel these motives. Thus: (1) the agency and proprietary cost motives should be studied together, and (2) data from *publicly disseminated* financial statements have limitations for drawing inferences about either of these motives for nondisclosure. Hence, the confidential Census data at the disaggregated operating level are a key advantage for this study.

The second advantage of the Census data is the ability to examine public and private U.S. establishments, allowing us to more accurately measure industry-level phenomena that affect disclosure decisions. In addition, we are able to develop a new proprietary cost measure that reflects the portion of total industry sales made by private firms. We use this variable to test whether public firms in industries with high concentrations of private competitors are more likely to mimic the nondisclosure policies of their private peers.

We evaluate proprietary and agency cost motives for nondisclosure. Prior studies of segment reporting (e.g., Harris 1998; Botosan and Stanford 2005) have focused on the proprietary cost motive, which posits that nondisclosure occurs to conceal proprietary information from competitors, suppliers, or regulators. Although the proprietary cost motive has received considerable attention, there is no clear consensus regarding its descriptive validity due to three main issues.

First, the evidence is conflicting. For example, consider the evidence regarding the role of industry concentration, a commonly used measure of product market competition. Bamber and Cheon (1998) find that whether a firm *provides* earnings forecasts is negatively related with industry concentration; however, Verrecchia and Weber (2006) find that whether a firm asks the Securities and Exchange Commission (SEC) to *withhold* proprietary information from its filings is also negatively related to industry concentration. Hence, the former study concludes that there is *less* informative disclosure in more concentrated industries, while the latter study concludes that there is *more*.

Second, there are concerns about the way proprietary cost proxies have been measured. Again, consider product market competition proxies. Typically, these proxies are calculated using Compustat data. However, Ali et al. (2009) show that industry concentration measures calculated using Compustat data are weak proxies for total (public and private firm) industry concentration. They show that using Census industry concentration measures often reverses prior results (including research on segment reporting) that were based on Compustat data.

Finally, Berger and Hann (2007) argue that much of the extant evidence consistent with the proprietary cost hypothesis is also consistent with an alternative agency cost hypothesis that disclosures are withheld due to manager-shareholder conflicts of interest. Berger and Hann's (2007) findings are consistent with the agency cost hypothesis, but are at best mixed with respect to the proprietary cost hypothesis.

We develop measures for both proprietary and agency costs and examine the relative strength of each after controlling for several "non-strategic" determinants of disclosure. We use a sample of 1,625 firm-years from 1987, 1992, and 1997, with coverage in both Compustat and the Census's Longitudinal Research Database (LRD). Our analyses are conducted using logistic regressions at the "pseudo-segment" level. We aggregate all of the firms' LRD plants within the same four-digit Standard Industrial Classification (SIC) code together into one pseudo-segment. The dependent variable is an indicator equal to 1 for pseudo-segments with four-digit SIC codes that match either the primary or secondary SIC code of a disclosed segment for that firm on Compustat ("disclosed" pseudo-segments), and 0 otherwise ("hidden" pseudo-segments).

We analyze both single- and multi-segment firms, with our focus on the multi-segment firms for three reasons. First, consistency with prior work. Second, we argue that multi-segment firms have greater motive and opportunity to aggregate. Finally, our empirical results are consistent with the previous argument, as they suggest that managers of multi-segment firms behave more strategically in their aggregation decisions than do managers of single-segment firms.

Our most robust results within the multi-segment sample are that the likelihood a pseudo-segment is disclosed separately is negatively related to: (1) whether the pseudo-segment receives inefficient transfers of funds from the remainder of the firm, and (2) the speed of abnormal profit adjustment exhibited by firms in the pseudo-segment's industry.

The negative relation between inefficient transfers and the likelihood that a pseudo-segment is separately disclosed is consistent with the agency cost motive: managers suppress information about inefficient internal capital transfers. The interpretation of the relation between the likelihood of disclosure and the industry speed of abnormal profit adjustment is less clear-cut. To understand better the relation between the likelihood of disclosure and the industry speed of profit adjustment, we separate our multi-segment sample into observations that have value-reducing diversification programs (i.e., negative excess value firms) and observations that have value-enhancing diversification programs (i.e., nonnegative excess value firms), estimating our model separately for each of these subsamples.[1] We show that the industry speed of abnormal profit adjustment is negatively related to the probability of pseudo-segment disclosure in the subsample with value-enhancing diversification but not in the subsample with value-reducing diversification. Given that the value-enhancing subsample is less likely to contain firms with agency cost motives for pseudo-segment aggregation, the results support the proprietary cost interpretation discussed in Harris (1998).

Within the multi-segment sample, we also find that pseudo-segment disclosure is positively associated with the pseudo-segment's industry-adjusted profitability and negatively associated with a labor power proxy that equals the ratio of aggregate industry wages to aggregate industry sales. The first result is consistent with the agency cost motive for nondisclosure, as it implies that information about less profitable operations is hidden. The second result is consistent with the proprietary cost motive, as it implies that firms act in their shareholders' interest by withholding information from other rent-seeking stakeholders. While both results are manifest in our main set of tests, they are not robust to some of our sensitivity tests. In contrast, the results related to the industry speed of abnormal profit adjustment and to inefficient transfers are robust.

We also analyze single-segment firms and find results suggesting their managers behave less strategically than those of multi-segment firms when making disclosure decisions. When single-segment firm managers are strategic, their nondisclosure is to avoid revealing information to private competitors. In particular, for our single-segment sample, only one of our agency/proprietary cost proxies is associated with the likelihood of disclosure. This proxy equals the proportion of industry sales attributable to privately held firms, and it is negatively associated with the probability of disclosure. This result offers a potentially new insight about the nature of proprietary costs, as it suggests that publicly traded firms seek to minimize disclosure levels when they compete with privately held firms that have no legal duty to publicly disclose.

Section II discusses our hypotheses. Section III presents our sample selection, data description, and research design. Results are discussed in Sections IV and V, with concluding comments in Section VI.

II. HYPOTHESIS DEVELOPMENT

Based on models such as those in Verrecchia (1983) and Hayes and Lundholm (1996), prior empirical studies have explored whether proprietary information costs reduce disclosure of industry segments via aggregation. The evidence is mixed. Harris (1998) finds that operations in more concentrated industries are less likely to be reported as separate industry segments, but Ali et al. (2009) show that Harris's finding is attributable to the use of Compustat data, which generally exclude privately held firms, to measure concentration. Botosan and Harris (2000) find no association between proprietary costs and voluntary increases in segment disclosure frequency. Botosan and Stanford (2005) find that managers hide profitable segments operating in less competitive industries, but Berger and Hann (2007) generally do not find such results.

Managers may also face agency costs of segment disclosure, because disaggregated segment data can provide information indicative of unresolved agency problems. Segment data provide information about a company's diversification strategy and its transfers of resources across divisions. Prior research finds evidence consistent with internal capital markets in conglomerates transferring funds across segments in a suboptimal manner (Berger and Ofek 1995; Lamont 1997; Shin and Stulz 1998; Rajan et al. 2000). Several studies indicate that diversified firms trade at a discount relative to stand-alone firms (Lang and Stulz 1994; Berger and Ofek 1995) and that the diversification discount is associated with measures of agency problems (Denis et al. 1997; Berger and Ofek 1999).

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[1] We consider a multi-segment firm's diversification program to be value-reducing (value-enhancing) if the excess value measure developed in Berger and Ofek (1995) is less than zero (nonnegative). The excess value measure represents an estimate of diversification's effect on firm value.

Berger and Hann (2003) find that firms moving to more disaggregated segment reporting under SFAS No. 131 experienced an increased diversification discount, implying that managers concealed information about agency problems under SFAS No. 14.[2] The same implication arises from Bens and Monahan (2004), who find that greater voluntary segment disaggregation is associated with a smaller diversification discount. Berger and Hann (2007) test this implication and find evidence consistent with the withholding of segment data being motivated by the desire to conceal agency problems.

III. SAMPLE SELECTION, DATA, AND METHODOLOGY

Sample Selection and Data

Our sample selection begins with all annual firm observations from the years 1987, 1992, and 1997 for which the firm is listed on Compustat's Annual Industrial, Research, or Full Coverage files and also covered within the Longitudinal Research Database (LRD), maintained by the Center for Economic Studies at the Bureau of the Census. The LRD is made up of two databases. The first is the *Census of Manufacturers* (*CM*), which is conducted every five years. The unit of analysis is a manufacturing plant, which the Census refers to as an *establishment*. Extremely small establishments are excluded from the *CM*, although the Census imputes estimates for them based on Internal Revenue Service and Social Security Administration data; all other establishments are required by law to respond truthfully to the *CM* (U S. Code Title 13, §224). The second LRD database is the *Annual Survey of Manufacturers* (*ASM*), which is conducted in all non-*CM* years. Through the end of our sample period, all establishments with more than 250 employees as of the most recent *CM* are included in the *ASM* panel.[3]

We use only the *CM* years to ensure coverage of nearly all of establishments in each industry. Because many of our independent variables are industry-based, this decreases measurement error. It also makes our research more comparable to studies that use *publicly available* industry measures from the *CM* available on the Census website. Although such measures are limited to a few industry-level variables such as the four-firm concentration index and the Herfindahl concentration index, recent studies such as Ali et al. (2009) and Tang (2009) demonstrate the importance of including private firms in these measures.

The sample begins in 1987 because that is the first *CM* year for which we have Compustat segment data and for which Compustat reports segment SIC codes. The sample ends in 1997 because that is the last year in which firms report segments under SFAS No. 14 rules and also the last year in which Census classified industries using SIC codes (NAICS codes are now used). Ending the sample with the completion of the SFAS No. 14 regime allows us to restrict our attention to *industry-based*, as opposed to *internal-management*, segment reporting. Nevertheless, our findings are likely to also apply to the SFAS No. 131 regime, as it continues to allow managers to have discretion in aggregating plant-level data into reportable segments and because the vast majority of the internal-management segments under SFAS No. 131 turn out to be based on either industry or geography. More importantly, our goal is to use segment reporting to examine discretion in financial reporting aggregation generally. The greater discretion available to managers under SFAS No. 14 is thus a desirable feature for our research.

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[2] SFAS No. 14 is FASB Statement No. 14, *Financial Reporting for Segment of a Business Enterprise* (FASB 1976). SFAS No. 131 is FASB Statement No. 131, *Disclosures about Segments of an Enterprise and Related Information* (FASB 1997).

[3] Comprehensive descriptions of the LRD are provided by McGuckin and Pascoe (1988) and the website of the Center for Economic Studies (http://www.ces.census.gov).

We aggregate all of the firm's establishments that operate within the same four-digit SIC code together into one "pseudo-segment." Sample firms must have *at least two* pseudo-segments (i.e., operate multiple plants with at least two unique four-digit SIC codes). This ensures that all sample firms have some potential for aggregation of pseudo-segments into financial reporting segments, as well as the opportunity to transfer resources across pseudo-segments.

Because the LRD covers only U.S. manufacturing establishments, we require that the firm be domiciled in the U.S. and that its primary SIC code on Compustat fall within the manufacturing sector (i.e., between 2000 and 3999). Even so, many firms have a portion of their production that occurs either outside the U.S. or outside of manufacturing. Moreover, while Compustat correctly eliminates intersegment sales within a firm, some double-counting of interplant sales occurs in the Census data. Therefore, the last step in our sample selection process eliminates observations for which there is a poor match between the firm's sales per the LRD and its sales per Compustat. In particular, we remove observations when the ratio of the firm's LRD *total value of shipments* (*TVS*) to Compustat sales is less than 0.75 or greater than 1.25. *TVS* is the Census term for sales.[4] The ratio of *TVS* to sales is below 0.75 if the firm has significant non-manufacturing sales or it has production facilities outside the U.S. generating sales. The ratio exceeds 1.25 if intersegment sales have not been eliminated properly by Census.

Dependent Variable

Following Harris (1998), we evaluate a dichotomous dependent variable, *MATCH*, that captures whether a pseudo-segment is disclosed as a separate external segment. In particular, *MATCH* equals 1 if the pseudo-segment's four-digit SIC code per the Census matches the primary or secondary SIC code of one of the firm's line-of-business segments per Compustat, and 0 otherwise. Under SFAS No. 14, enterprises were required to classify line-of-business segment information using the *industry approach*. Thus, we assess the extent to which a four-digit SIC-based classification of industries for all the firm's plant-level data match the externally reported industry segments chosen by management.[5]

Segment disclosure has been a contentious issue for decades. Researchers' interest derives from the fact that, while SFAS No. 14 required disclosure about firms' operations in different industries (paragraph 1), it explicitly stated that no single industry classification system (such as the SIC code) "is, by itself, suitable to determine industry segments for purposes of this Statement" (paragraph 12). Paragraph 12 continues with the following:

> [N]o single set of characteristics is universally applicable in determining the industry segments of all enterprises, nor is any single characteristic determinative in all cases. Consequently, determination of an enterprise's industry segments must depend to a considerable extent on the management of the enterprise.

The discretion afforded to management is what makes segment reporting an interesting disclosure choice. However, this discretion also makes it difficult for researchers to determine which industries *should* be disclosed. In our setting, we rely on further guidance from SFAS No. 14 to generate predictions of unbiased disclosure. Specifically, paragraph 13 states:

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[4] *TVS* is defined as the "selling value f.o.b. plant after discounts and allowances and excluding freight charges" (*Value of Product Shipments: 2005—Annual Survey of Manufactures*, Appendix A, issued November 2006).

[5] We are interested only in the line-of-business (LOB) segment reporting choice for two reasons. First, as a practical matter, the LRD data cover only U.S. manufacturing plants and the extent of geographical diversification in these data is thus limited to within-U.S. variation. Second, with the exception of Hope and Thomas (2008), who focus on geographic diversification, the extant literature has focused on agency problems within the context of line-of-business diversification.

> An enterprise's existing profit centers—the smallest units of activity for which revenue and expense information is accumulated for internal planning purposes—represent a logical starting point for determining the enterprise's industry segments.

The definition of *profit center* coincides with Census's choice to collect plant-level data. This, in turn, justifies our maintained assumption that a pseudo-segment (i.e., a group of profit centers sharing the same four-digit SIC code) is the starting point from which management uses its discretion under SFAS No. 14 when deciding whether to aggregate the pseudo-segment with others or to report it separately in the segment footnotes. We are not asserting that each pseudo-segment *should* be reported separately; rather, our maintained assumption is that each pseudo-segment *has the potential* to be reported separately.

Given the FASB explicitly states that four-digit SIC codes are not a sufficient basis for *all* firms to determine their reportable segments, our aggregation of plants by common four-digit SIC code into pseudo-segments is *ad hoc*. On the other hand, the FASB provides no preferable alternative. Moreover, defining industries with SIC codes has a long history in the literature, with Bhojraj et al. (2003) noting that SIC codes have been available since 1939 and that more than 90 percent of studies that use a general industry classification scheme use SIC codes despite their shortcomings. Thus, we believe it reasonable to rely on this classification scheme while also recognizing that the use of four-digit SIC codes may introduce measurement error. Hence, in sensitivity analyses we: (1) report a validity check of our disclosure measure, *MATCH*, using a comparison based on the data set constructed by Berger and Hann (2003; 2007) and (2) conduct a sensitivity check in which we define industries on the basis of three-digit SIC codes.

Model of Managers' Segment Reporting Choice

We examine managers' segment reporting decisions by estimating, at the pseudo-segment level, the logit regression tabulated in Table 4. The dependent variable, *MATCH*, is a dichotomous variable with the value of 1 if the pseudo-segment's four-digit SIC code per the Census matches a primary or secondary SIC code of a Compustat line-of-business segment reported by the firm, and 0 otherwise.

Treatment Variables

I_PROFIT is a ratio that measures abnormal profitability. The numerator equals the pseudo-segment's gross margin percentage less the average gross margin percentage for all establishments in its industry.[6] The denominator is the standard deviation of the gross margin percentage for all establishments in the pseudo-segment's industry. *I_PROFIT* does not fully capture *total* pseudo-segment profitability (i.e., it ignores some expenses and, more importantly, asset turnover). However, by industry-adjusting, we reduce the likelihood of our profit measure having a low correlation with a total profitability measure such as return on assets (ROA). The rationale is that the negative correlation typically observed between total asset turnover and gross margin is smaller within industries than across industries. We do not attempt to calculate an ROA-based measure because the Census data do not contain sufficient information about establishment-level assets.

I_PROFIT is a direct measure of pseudo-segment profits that captures how well a pseudo-segment performs relative to its industry. It is pseudo-segment profits rather than publicly available firm-level (or external segment-level) profits that managers may try to aggregate. Thus, because it is based on confidential information that is not observable by outsiders, *I_PROFIT* is a superior proxy to publicly observable measures of profit. The coefficient relating *I_PROFIT* to disclosure will be positive if the agency cost hypothesis dominates and only strongly performing units tend to be disclosed. Conversely, the coefficient will be negative if the proprietary cost hypothesis dominates and strong performers are hidden.

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[6] Please refer to the notes to Table 2 for additional details regarding the construction of our treatment and control variables. Unless otherwise noted, the term "industry" refers to a group of firms, establishments, segments, or pseudo-segments that have the same four-digit SIC code.

PROFITADJ is an industry abnormal profit adjustment measure similar to a construct used by Harris (1998). It captures the speed with which those industry participants with above-average profits have their positive abnormal profitability revert to the industry mean. We estimate *PROFITADJ* for each industry using the industry-level panel regression over the period 1984 to 1997 that is described in the notes to Table 2.[7]

While prior literature documents a negative relation between *MATCH* and *PROFITADJ*, the interpretation of this result is unclear. Harris (1998) interprets the negative coefficient as a manifestation of proprietary costs: firms aggregate pseudo-segments from industries in which firms with above-average profits maintain their profitability advantage for long durations. However, a negative coefficient is also consistent with agency costs. That is, rather than the hidden pseudo-segments predominantly representing businesses with better profitability than their industry peers, firms may hide operations with below-average performance from shareholders because these operations are in an industry in which other firms are able to consistently achieve above-average performance.

PRIVATE is a proprietary cost proxy. Privately held U.S. firms have no obligation to publicly reveal financial results. Thus, we predict that the publicly traded firms in our sample competing in industries with high concentrations of private firms will tend to mimic the disclosure policies of their private competitors.[8] We classify a Census establishment as private if it cannot be linked to a Compustat firm. We then calculate *PRIVATE* as the ratio of total private-establishment *TVS* to total *TVS* within the pseudo-segment's industry for that Census year.[9] We predict a negative association between *PRIVATE* and the likelihood of pseudo-segment disclosure.

TRANSIN is an indicator variable equal to 1 (0) if the pseudo-segment receives (does not receive) inefficient transfers from the rest of the firm. To calculate *TRANSIN*, we adapt the procedures of Rajan et al. (2000) to our use of Census data, resulting in a three-step calculation. The first two steps produce our estimate of transfers made or received by each pseudo-segment. First, we take the difference between the pseudo-segment's actual investment and the average investment level in its four-digit SIC code, with investment defined as the ratio of net new plants (i.e., plants added less plants removed) to end-of-period plants for each five-year Census period. This is the *industry-adjusted investment ratio* (*IAIR*).

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[7] To increase the precision of our estimates, we estimate *PROFITADJ* using the entire panel of data. However, this suggests that the manager making the disclosure decision in 1987 or 1992 can predict the profit persistence in future years. Our conclusions are unchanged if we relax this assumption and estimate *PROFITADJ* using only historical data (i.e., for 1987 using 1984–1987, for 1992 using 1984–1992, and for 1997 using the entire panel).

[8] A related interpretation of *PRIVATE* is that it is inversely related to barriers to entry in the industry, which may also be related to proprietary incentives to withhold information. Specifically, if industries with more privately held firms require less capital to enter (hence, the ability of private firms to stay private and avoid the capital markets), the incumbent publicly traded firms have an incentive to withhold disclosure from potential new entrants. We separately capture low barriers to entry in one of our other explanatory variables, *LOENT_HISUB*, and we find that *PRIVATE* has only a modest positive correlation with *LOENT_HISUB*. Thus, we view *PRIVATE* as primarily capturing the extent to which required disclosure by competitors is low.

[9] This variable contains three potential sources of measurement error. First, the matching of Compustat observations to observations in the Census's LRD is imperfect. In particular, these matches are based on the Census's Compustat-LRD Bridge File, which contains nonpermanent Compustat identifiers taken from an edition of Compustat that is older than the one we are using. Second, some of the plants that we classify as private may be owned by non-U.S. *publicly* traded firms that make information available as part of their home-country security laws and accounting principles. Finally, we ignore sales attributable to plants located outside the U.S. when calculating the ratio.

Second, we improve the initial estimate to account for a selection effect. Specifically, the relatively large publicly traded firms in our sample likely have more funds available than the average firm included in the LRD given the inclusion of many privately held, relatively small firms in this database. The likelihood of relatively high costs of capital for the industry average firm relative to our sample firms means that the difference between a sample pseudo-segment's investment and the industry average will tend to overstate the amount of funds transferred between pseudo-segments. To correct for this selection effect, we follow Rajan et al. (2000) by subtracting from each pseudo-segment's *IAIR* the sales-weighted average of the *IAIR*s across all the pseudo-segments of the firm. This is the *industry-firm-adjusted investment ratio* (*IFAIR*).

Finally, the third step is to set *TRANSIN* equal to 0 for pseudo-segment j if: (1) $IFAIR_j$ is negative (i.e., pseudo-segment j is *making* transfers), or (2) $IFAIR_j$ is positive but the transfer received is deemed efficient. On the other hand, if $IFAIR_j$ is positive *and* the transfer received is deemed inefficient, *TRANSIN* is set equal to 1 for pseudo-segment j. We determine whether to deem a transfer as efficient or inefficient by comparing the industry Tobin's q of pseudo-segment j to the average q for all other pseudo-segments of the firm. If the transfer recipient's q is below (above) the firm average, then the transfer is deemed inefficient (efficient).[10] We predict a negative association between *TRANSIN* and the likelihood of pseudo-segment disclosure. Based on the agency cost hypothesis, we expect that firms over-investing in low-growth-opportunity pseudo-segments are less likely to separately disclose these units in their external reports.

LOENT_HISUB is an indicator variable measuring proprietary costs related to industry competition. Analytical predictions on the relation between discretionary disclosure and industry competition depend on the nature of the competition, including whether the competition is from potential entrants or incumbents and whether firms compete on setting quantity or price. Thus, one-dimensional measures of industry competition such as concentration ratios do not have clearly predictable relations to discretionary disclosure. We therefore construct *LOENT_HISUB* as a multidimensional measure of industry competition.

The *LOENT* term refers to low entry barriers, which indicate more competition from potential entrants. The *HISUB* term refers to high product substitutability, which indicates more competition from incumbents. We use the methods described in Tang (2009) to measure both phenomena. We use the ratio of capital expenditures deflated by revenues for an industry as our measure of barriers to entry arising from the need for capital investment. We measure product substitutability as the ratio of total revenue divided by the sum of raw materials and payroll costs for an industry, thus capturing the price-to-cost margin for the industry (which should be inversely related to the extent of product substitutability). Low values of the entry barrier ratio combined with high values of the product substitutability ratio indicate a pseudo-segment with high competition from both potential entrants and incumbents. Hence, we set *LOENT_HISUB* equal to 0 *unless* the former ratio is below the median value of our sample industries *and* the latter ratio is above the median value of our sample industries, in which case we set *LOENT_HISUB* equal to 1. If competition discourages discretionary disclosure, then *LOENT_HISUB* will have a negative association with pseudo-segment disclosure.

LABOR measures the ratio of total labor costs to total sales revenues for an industry. Little attention has been given to proprietary costs of disclosing information to customers or suppliers.

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[10] We compute q ratios for 1985, 1990, and 1995 for each single-segment Compustat firm in every four-digit SIC code that has at least one Compustat segment in our sample in that year. Our q ratios are computed using the Lindenberg and Ross (1981) methodology and the specific assumptions of Hall et al. (1988). We then assign to each pseudo-segment for each five-year investment window the industry median q ratio of the Compustat single-segment firms that operate in the same industry as the pseudo-segment. We use the finest industry match that provides at least five single-segment Compustat firms with which to calculate the industry median q.

LABOR captures proprietary costs of disclosing information to suppliers of labor under the maintained assumption that these proprietary costs are greater when labor captures a larger fraction of the firm's value-added (Liberty and Zimmerman 1986; Scott 1994). If management wishes to obfuscate the true performance of the firm in order to maintain an information advantage over labor, then there will be a negative association between *LABOR* and *MATCH*.

Control Variables

Prior research indicates that greater size is generally associated with a higher level of disclosure (e.g., Lang and Lundholm 1993). We measure *FSIZE* as the natural logarithm of total firm assets (in $ millions) from Compustat.

The industry four-firm concentration ratio, *FOURFIRM*, equals the fraction of aggregate industry *TVS* that is attributable to the four industry establishments with the largest *TVS*. Higher values of *FOURFIRM* imply more industry concentration. We match the industry measure to each pseudo-segment by four-digit SIC code. Harris (1998) uses this ratio calculated from Compustat data as a measure of competition and finds that it is negatively associated with the likelihood of disclosure. Conversely, Ali et al. (2009) find no association between a Census-based concentration ratio and the decision to provide segment disclosures.

The next three control variables capture the firm's ability to aggregate the pseudo-segment within a financial statement segment. *RELSIZE* is the ratio of pseudo-segment *TVS* to firm *TVS*. We expect a higher likelihood of pseudo-segment disclosure if the pseudo-segment constitutes a bigger share of firm operations; hence, we predict a positive association between *RELSIZE* and *MATCH*. *SEGDIVERSITY* is a measure of diversity. Under SFAS No. 14, firms with operations in similar industries were afforded greater discretion to aggregate segment information. We measure segment diversity as the ratio of the number of unique two-digit SIC codes across pseudo-segments to the total number of pseudo-segments. We predict a positive association between *SEGDIVERSITY* and *MATCH*. Finally, as an additional measure of firm complexity, we include a count of the number of pseudo-segments that the firm operates in, *NUMEST*. As the count of pseudo-segments increases, aggregation of information is more likely. We thus expect a negative association between *NUMEST* and *MATCH*.

Recall that we eliminate all observations where firm-level *TVS* per the Census is either below 75 percent, or above 125 percent, of firm-level Compustat sales. This reduces measurement error, but does not eliminate it. We therefore include *CEN_CMPSTAT*, which equals the ratio of firm-level *TVS* per the Census to firm-level sales per Compustat. As this variable increases, we gain greater comfort that all corporate activity per Compustat is being captured by the Census (indicating that the firm's operations are concentrated in domestic manufacturing). Hence, there is a greater likelihood that any individual pseudo-segment will be disclosed and we expect a positive coefficient on *CEN_CMPSTAT*.

Firms report a numeric four-digit SIC code on their Census survey. However, when reporting externally, they use a textual industry description that S&P converts to a four-digit SIC code for Compustat. We use two variables that capture economic forces related to industry membership that also control for measurement error in the S&P coding. First, we calculate *INDMATCHRATE*, which equals the proportion of pseudo-segments in an industry where *MATCH* equals 1, excluding the sample pseudo-segment being analyzed. *INDMATCHRATE* captures the measurement error that results if S&P systematically misclassifies certain industries. We predict a positive association between *INDMATCHRATE* and *MATCH*.[11]

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[11] This variable also captures a strategic aspect of segment disclosure. When an industry has a lower proportion of its establishments being separately disclosed as external segments, the proprietary costs of disclosing an establishment from that industry as a separate segment are likely higher. The proprietary cost hypothesis therefore also leads us to predict

We also create an indicator variable, *NEC*, for any pseudo-segment with a four-digit SIC code described as "Not Elsewhere Classified" in the SIC manual. These are potentially idiosyncratic operations where S&P might err when converting a text description to a numeric one. Even if S&P does not err, it may be easier for firms to argue that an idiosyncratic operation is not part of a separate industry, but instead should be aggregated into a broader reported segment. We predict a negative association between *NEC* and *MATCH*.

IV. RESULTS FOR MANAGERS' SEGMENT REPORTING DECISIONS

Descriptive Statistics and Sample Distribution

Panel A of Table 1 details the Compustat observations lost at each step in our sample selection and data validation processes. Note that Panels A and B of Table 1 present data at the firm level (rather than the pseudo-segment level at which our analyses are performed). Specifically, these two panels provide information on the data requirements for Compustat firms to remain in our sample (Panel A) and on the extent of within-firm disaggregation based on Census pseudo-segments versus Compustat segments (Panel B).

Panel A of Table 1 shows that we begin with 9,975 Compustat observations from the years 1987, 1992, and 1997. We lose 4,195 observations that either cannot be linked to an observation in the Census's LRD (3,072 observations) or that have incomplete Census data (1,123 observations).[12] We eliminate an additional 2,667 observations that are on both Compustat and the LRD in 1987, 1992, or 1997 because the observation does not meet our requirement of having at least two pseudo-segments. Finally, we remove 323 observations where firm-level *TVS* per the LRD exceeds 125 percent of firm-level sales per Compustat and 1,165 observations where the firm-level *TVS* is less than 75 percent of firm-level sales. These steps result in a final sample of 1,625 observations (representing 1,008 unique firms).

Table 1, Panel B contains descriptive statistics about the degree of disaggregation. The average number of Census pseudo-segments per firm, at 5.1, is more than three times the corresponding average of 1.6 Compustat segments per firm and the standard deviation of pseudo-segments per firm is more than five times the standard deviation of segments per firm. Thus, even after grouping plants by four-digit SIC code, the Census data exhibit a greater level of, and more variability in, disaggregation than the Compustat segment data.

Table 1, Panel C presents the sample's distribution by two-digit SIC code and compares it to that of the two-digit manufacturing SIC codes on Compustat.[13] The first two columns of numbers in Panel C show that pseudo-segments are fairly evenly distributed across industries, with only SIC codes 34 (Fabricated Metals and Transportation Equipment), 35 (Industrial/Commercial Machinery and Computers), and 36 (Electrical Equipment) having more than 10 percent of our sample's pseudo-segments. In contrast, *TVS* is somewhat concentrated in combined SIC codes 20

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a positive relation between *INDMATCHRATE* and *MATCH*.

[12] Failures to match Compustat to LRD data occur for at least two reasons. First, we are using Compustat data that were matched to the LRD by Census staff on the basis of name and address. Names in the Census data may represent divisions and not ultimate parents and thus the firm may not be matched. Second, the original match was completed by Census in 2001 and CUSIP was used as the identifying variable in Compustat. If the firm changed its CUSIP (or S&P reassigned the CUSIP) after 2001, the firm will not match an observation in the edition of Compustat that we use. We compare the Compustat median sales (assets) data for matched and unmatched firms and find (in untabulated results) that the matched firms are more than three (two) times larger than unmatched firms. Matched firms have median sales (assets) of \$102.5 (\$82.2) million, while unmatched firms have median sales (assets) of \$32.8 (\$39.2) million. In addition to being smaller, we also find that the unmatched firms are less profitable (median return on sales of 2.7 percent versus 4.0 percent for the matched observations). The fact that unmatched firms are smaller corresponds to a finding reported by Maksimovic et al. (2008).

[13] Census disclosure requirements caused us to combine two industries in all situations where one of the industries exhibited fairly small representation.

TABLE 1

Comparison of Final Sample to Compustat

Panel A: Reconciliation of Compustat Sample and Final Sample

	Firm-Years
In Compustat	9,975
Less:	
Not in the Census Compustat bridge file	4,195
Has only one pseudo-segment	2,667
Aggregate *TVS* per census exceeds 125 percent of *SALES* per Compustat	323
Aggregate *TVS* per census is less than 75 percent of *SALES* per Compustat	1,165
Final Sample	1,625

Panel B: Firm Level Descriptive Statistics

Pseudo-Segments			Compustat Segments		
Mean	Median	Standard Deviation	Mean	Median	Standard Deviation
5.1	3	5.1	1.6	1	0.9

Panel C: Industry Composition

		Sample		Compustat Manufacturing		All Compustat	
Two-Digit SIC Code	Industry Description	Percentage of Pseudo-Segments	Percentage of *TVS*	Percentage of Segments	Percentage of Sales	Percentage of Segments	Percentage of Sales
20 and 21	Food and Tobacco Products	5.94	18.17	5.20	11.66	5.41	12.12
22	Textiles	3.63	3.06	1.84	0.94	1.85	0.92
23	Apparel and Other Finished Products	2.70	1.86	2.09	0.90	2.01	0.88
24	Lumber and Wood Products	3.55	2.29	1.75	1.27	1.79	1.25
25	Furniture and Fixtures	2.67	1.55	1.47	0.71	1.44	0.68

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Panel C: Industry Composition

Two-Digit SIC Code	Industry Description	Sample: Percentage of Pseudo-Segments	Sample: Percentage of *TVS*	Compustat Manufacturing: Percentage of Segments	Compustat Manufacturing: Percentage of Sales	All Compustat: Percentage of Segments	All Compustat: Percentage of Sales
26	Paper and Allied Products	4.45	7.22	2.57	2.57	2.61	2.61
27	Printing and Publishing	4.42	5.92	2.83	1.38	3.69	2.03
28 and 29	Chemicals and Petroleum	5.73	6.88	14.46	20.41	14.30	20.54
30 and 31	Rubber, Plastics, and Leather	8.06	3.08	4.27	1.81	4.29	1.82
32	Stone, Clay, Glass, and Concrete	2.78	1.35	1.93	0.87	1.98	0.96
33	Primary Metals	5.76	6.42	3.85	3.84	3.87	3.81
34	Fabricated Metals and Transportation Equipment	11.34	4.73	4.86	1.51	5.00	1.55
35	Industrial/Commercial Machinery and Computers	13.33	6.26	16.65	13.28	16.38	12.81
36	Electrical Equipment	12.20	9.11	15.05	10.93	14.67	11.10
37	Transportation Equipment	5.35	16.55	5.35	22.57	5.28	21.53
38	Measuring Instruments, Photo Goods, and Watches	6.42	4.71	13.29	4.77	12.88	4.82
39	Miscellaneous	1.67	0.84	2.54	0.58	2.55	0.58
Total		100.00	100.00	100.00	100.00	100.00	100.00

Panel A provides a reconciliation of all observations on Compustat for the years 1987, 1992, and 1997 and the observations that make up our sample of 1,625 firm-years (1,008 unique firms) for the years 1987, 1992, and 1997. A firm is included in our sample if its primary SIC code is in the manufacturing sector (SIC 2000–3999) and it can be matched from Compustat to the Longitudinal Research Database (LRD) of the U.S. Census Bureau.

Panel B provides descriptive statistics for our sample.

Panel C compares the industry composition of the pseudo-segments in our sample to the industry composition of Compustat line-of-business segments. In Panel C All Compustat refers to a sample of 13,530 segments (10,442 firm-years, 5,863 firms) that have non-negative sales and assets per the Compustat database for the years 1987, 1992, or 1997, and have a primary or secondary SIC code between 2000 and 3999. Compustat Manufacturing relates to 12,358 segments (9,585 firm-years, 5,441 firms) that are a subset of All Compustat and for which 75 percent of firm-level sales is attributable to manufacturing segments (i.e., segments with either a primary or secondary four-digit SIC code between 2000 and 3999).

(continued on next page)

A *pseudo-segment* is defined as all LRD plants of a firm that operate in the same four-digit SIC code. *TVS* denotes total value of shipments for a particular pseudo-segment per the LRD. In Panel A *Aggregate TVS* is defined as follows:

$$Aggregate\ TVS = \sum_{j=1}^{n} TVS_j.$$

In the above equation j is a pseudo-segment index and n is the number of pseudo-segments for a particular firm. *SALES* denotes annual sales per Compustat (i.e., data item 12). ***Compustat segments*** reflect line-of-business segments reported by the firm in its external reports filed with the SEC. In Panel C the percentage of pseudo-segments represents the percentage of all sample pseudo-segments in a particular industry; percentage of *TVS* represents the percentage of the total value of shipments reported by all pseudo-segments that is attributable to the pseudo-segments of a particular industry; percentage of segments represents the percentage of all manufacturing segments in a particular industry; percentage of sales represents the percentage of all sales reported by manufacturing segments that is attributable to segments of a particular industry.

and 21 (Food and Tobacco Products), and in SIC code 37 (Transportation Equipment).

The middle two columns of numbers in Table 1, Panel C contain the industry distribution for Compustat segments reported by firms for which at least 75 percent of the firm's sales come from manufacturing industries, whereas the final two columns of the panel present the industry distribution for all Compustat segments that have primary or secondary SIC codes between 2000 and 3999. The two sets of Compustat figures are very similar and reveal that, although the industry distribution of our sample is broadly similar to that of the Compustat population, our sample is underrepresented in the Chemicals and Petroleum industry and overrepresented in the Fabricated Metals and Transportation Equipment industry.

Univariate Statistics

In Table 2, we examine how descriptive statistics for our key explanatory variables differ between the observations for which the dependent variable, *MATCH*, equals 1 and those for which it equals 0. The unit of analysis in this table, and in the remaining tables, is the pseudo-segment. We divide Table 2 into three panels that capture three different samples.

The full sample (Table 2, Panel A) includes all 1,625 firm-year observations, and thus all 8,287 of our sample pseudo-segments. The single-segment sample (Panel B) contains the 956 firm-year observations (3,300 pseudo-segments) for which the firm reports either one line-of-business segment or has no line-of-business segment footnote in its 10-K. Finally, the multi-segment sample (Panel C) contains the 669 firm-year observations (4,987 pseudo-segments) in which the firm reports two or more line-of-business segments. In each of the three samples, differences in the values of our explanatory variables emerge between the observations for which *MATCH* equals 1 and those for which it equals 0. All differences discussed below (both means and medians) are statistically significant at the 1 percent level unless otherwise noted.

For the full sample results presented in Table 2, Panel A, approximately 37 percent of the pseudo-segments are disclosed as Compustat segments (i.e., *MATCH* equals 1 for 3,032 of 8,287 observations). The median value of *I_PROFIT* is 0.08 for undisclosed pseudo-segments and 0.11 for disclosed pseudo-segments. This represents a 38 percent difference and is consistent with the agency cost motive, as it suggests that pseudo-segments are more likely to be aggregated when abnormal profitability is lower.

PROFITADJ is the *industry* rate of positive abnormal profit adjustment, where profit is based on gross margin percentage. The higher mean and median values of *PROFITADJ* for the non-disclosing sample indicate a slower speed of positive abnormal profit adjustment for the industries of non-disclosing pseudo-segments, consistent with the findings in Harris (1998), Botosan and Stanford (2005), and Berger and Hann (2007).

The 7 percent higher mean and median values of *PRIVATE* for the non-disclosing sample indicate that undisclosed pseudo-segments are from industries with higher concentrations of private firms. The mean values of *TRANSIN* indicate that 32 percent of undisclosed pseudo-segments receive inefficient transfers, whereas only 26 percent of disclosed pseudo-segments are recipients of such subsidies. Thus, consistent with the agency cost hypothesis, the univariate statistics indicate that pseudo-segments receiving subsidies are less likely to be disclosed.

LOENT_HISUB has a mean value of approximately 28 percent for both disclosed and undisclosed pseudo-segments. However, the mean and median values of *LABOR* are about 1 percent higher at undisclosed pseudo-segments, consistent with disclosure being less likely when salaries and wages represent a larger fraction of sales in the pseudo-segment's industry.

For brevity, we do not discuss the statistics related to our control variables, with the exception of *RELSIZE*, which is the ratio of pseudo-segment *TVS* to firm-level *TVS*. Untabulated results reveal that the median value of *RELSIZE* is 0.08 across the combined sample, indicating that the median pseudo-segment provides 8 percent of its firm's sales. Thus, the majority of the pseudo-

TABLE 2

Pseudo-Segment Level Descriptive Statistics

Panel A: All Sample Firms

	Mean		Median		Standard Deviation	
	MATCH = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0
I_PROFIT	0.15	0.05***	0.11	0.08***	0.69	0.85
PROFITADJ	0.32	0.35***	0.29	0.34***	0.39	0.37
PRIVATE	0.56	0.63***	0.58	0.65***	0.20	0.20
TRANSIN	0.26	0.32***	0.00	0.00***	0.44	0.46
LOENT_HISUB	0.28	0.28	0.00	0.00	0.45	0.45
LABOR	0.21	0.22***	0.21	0.22***	0.07	0.08
FSIZE	5.56	6.26***	5.49	6.29***	1.66	1.73
FOURFIRM	0.34	0.30***	0.31	0.26***	0.18	0.18
RELSIZE	0.36	0.10***	0.26	0.04***	0.30	0.17
SEGDIVERSITY	0.58	0.49***	0.50	0.46***	0.24	0.23
NUMEST	7.04	11.95***	5.00	8.00***	6.65	10.91
CEN_CMPSTAT	0.96	0.95***	0.96	0.94***	0.12	0.12
INDMATCHRATE	0.42	0.32***	0.41	0.33***	0.26	0.24
NEC	0.20	0.25***	0.00	0.00***	0.40	0.44
n	3,032	5,255				

Panel B: Single-Segment Firms

	Mean		Median		Standard Deviation	
	MATCH = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0
I_PROFIT	0.14	0.05***	0.11	0.07***	0.73	0.87
PROFITADJ	0.34	0.34	0.30	0.34	0.39	0.38
PRIVATE	0.55	0.63***	0.56	0.66***	0.20	0.20
TRANSIN	0.25	0.31***	0.00	0.00***	0.43	0.46
LOENT_HISUB	0.27	0.28	0.00	0.00	0.44	0.45
LABOR	0.20	0.21***	0.20	0.21***	0.07	0.08
FSIZE	4.99	5.61***	4.89	5.54***	1.43	1.65

(continued on next page)

Panel B: Single-Segment Firms

	Mean		Median		Standard Deviation	
	MATCH = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0
FOURFIRM	0.34	0.29***	0.31	0.26***	0.18	0.17
RELSIZE	0.53	0.16***	0.55	0.07***	0.31	0.21
SEGDIVERSITY	0.63	0.55***	0.50	0.50***	0.25	0.25
NUMEST	3.51	5.97***	3.00	4.00***	2.15	4.69
CEN_CMPSTAT	0.97	0.96**	0.98	0.97**	0.12	0.12
INDMATCHRATE	0.42	0.32***	0.41	0.33***	0.25	0.25
NEC	0.19	0.25***	0.00	0.00***	0.40	0.43
n	1,175	2,125				

Panel C: Multi-Segment Firms

	Mean		Median		Standard Deviation	
	MATCH = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0	*MATCH* = 1	*MATCH* = 0
I_PROFIT	0.15	0.05***	0.10	0.08***	0.67	0.84
PROFITADJ	0.30	0.35***	0.28	0.34***	0.39	0.36
PRIVATE	0.56	0.62***	0.59	0.65***	0.21	0.20
TRANSIN	0.27	0.32***	0.00	0.00***	0.44	0.47
LOENT_HISUB	0.29	0.28	0.00	0.00	0.45	0.45
LABOR	0.21	0.22***	0.21	0.22***	0.07	0.08
FSIZE	5.92	6.71***	5.92	6.82***	1.69	1.64
FOURFIRM	0.34	0.30***	0.31	0.26***	0.19	0.19
RELSIZE	0.25	0.06***	0.17	0.02***	0.24	0.11
SEGDIVERSITY	0.55	0.45***	0.50	0.40***	0.23	0.20
NUMEST	9.27	16.00***	7.00	13.00***	7.51	12.01
CEN_CMPSTAT	0.94	0.93***	0.94	0.92***	0.12	0.12
INDMATCHRATE	0.42	0.33***	0.41	0.33***	0.26	0.24
NEC	0.20	0.26***	0.00	0.00***	0.40	0.44
n	1,857	3,130				

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*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

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Panel A shows descriptive statistics for 8,287 pseudo-segments of 1,625 firm-years (1,008 unique firms) over the years 1987, 1992, and 1997.

Panel B shows descriptive statistics for 3,300 pseudo-segments of 956 firm-years over the years 1987, 1992, and 1997.

Panel C shows descriptive statistics for 4,987 pseudo-segments of 669 firm-years over the years 1987, 1992, and 1997.

The pseudo-segments described in Panel B all relate to firms that have one line-of-business segment per Compustat (i.e., single-segment firms). The pseudo-segments described in Panel C all relate to firms that have multiple line-of-business segments per Compustat (i.e., multi-segment firms).

Please refer to Table 1 for additional sample selection criteria.

A *pseudo-segment* is defined as all LRD plants of a firm that operate in the same four-digit SIC code. *MATCH* equals 1 if the pseudo-segment four-digit SIC code matches the primary or secondary SIC code of a Compustat segment, and 0 otherwise. *I_PROFIT* equals the difference between the pseudo-segment's gross margin and the industry average gross margin, divided by the standard deviation of gross margin across the industry; gross margin is obtained from the LRD database, and equals total value of shipments (*TVS*) less cost of materials less salaries and wages, with the difference scaled by *TVS*. In a regression estimated from all firms in the pseudo-segment's industry where the dependent variable is current year gross margin and the two independent variables are lagged gross margin if negative, and lagged gross margin if positive, the coefficient from the positive gross margin realizations is defined as *PROFITADJ*. *PRIVATE* equals the ratio of the sum of *TVS* across all firms in the pseudo-segment's industry that cannot be linked to Compustat (i.e., we assume that firms that are not on Compustat are privately held), divided by the sum of *TVS* across all firms in the industry. The variable *IFAIR* is used to calculate *TRANSIN*. *IFAIR* equals the proportion of a pseudo-segment's establishments that were not owned by the firm in the previous census year less the proportion of new plants for the entire industry (excluding the firm) less the weighted (by *TVS*) firm average of this difference across all of its pseudo-segments. In equation form:

$$IFAIR_j = \frac{NP_j}{EP_j} - \frac{NP^i_j}{EP^i_j} - \sum_{j=1}^{n} w_j\left(\frac{NP_j}{EP_j} - \frac{NP^i_j}{EP^i_j}\right).$$

In the equation, *NP* denotes new plants, *EP* ending plants, *w* is *RELSIZE* for the pseudo-segment, *j* indexes pseudo-segment, and *i* indexes the industry. *TRANSIN* equals 1 if *IFAIR* is positive for the pseudo-segment and the industry Tobin's Q for that pseudo-segment is less than the average of all other industry Tobin's Q measures for the firm, and 0 otherwise. *LOENT_HISUB* is an indicator variable that equals either 0 or 1. *LOENT_HISUB* equals 1 if the pseudo-segment is a member of an industry for which: (1) the ratio of industry-level capital spending to industry-level TVS in year *t* is below the year *t* median of all manufacturing industries, and (2) the ratio of industry-level *TVS* to the sum of industry-level raw materials costs and industry-level payroll costs for year *t* is above the year *t* median of all manufacturing industries. *LABOR* equals the ratio of total labor costs incurred by establishments in the same industry as the pseudo-segment to the sum of *TVS* for all establishments in the same industry as the pseudo-segment. *FOURFIRM* equals the sum of *TVS* for the four largest firms in the same industry as the pseudo-segment divided by the sum of *TVS* for all firms in the same industry as the pseudo-segment. *RELSIZE* equals the *TVS* of the pseudo-segment deflated by the total *TVS* summed across all of the firm's pseudo-segments. *SEGDIVERSITY* equals number of unique two-digit SIC codes across the firm's pseudo-segments deflated by the total number of pseudo-segments. *CEN_CMPSTAT* equals total *TVS* summed across all of the firm's pseudo-segments deflated by total sales of the firm per Compustat. *INDMATCHRATE* equals the proportion of pseudo-segments in an industry where *MATCH* equals 1, excluding the sample pseudo-segment being analyzed. *NEC* equals 1 if the four-digit SIC code contains the phrase "Not Elsewhere Classified" in the SIC manual. *NUMEST* equals the number of pseudo-segments operated by the firm. n denotes the number of observations.

segments represent a small enough portion of the firm to easily be aggregated within a financial statement segment under SFAS No. 14 rules requiring an industry segment to be separately reported if the segment's revenue, earnings, or assets are at least 10 percent of the combined total for that item across all of the firm's industry segments.[14]

Panel A of Table 2 shows that the median value of *RELSIZE* is 0.26 for disclosed pseudo-segments, versus 0.04 for those not separately disclosed as financial statement segments. This indicates that disclosed pseudo-segments generally contribute more than 10 percent of firm sales, a trigger point for separate segment disclosure under SFAS No. 14, whereas undisclosed pseudo-segments generally do not. Thus, it is critical to control for *RELSIZE* in our multivariate tests. Nevertheless, the large standard deviation for *RELSIZE* of 0.17 among undisclosed pseudo-segments indicates that a considerable portion of pseudo-segments with *RELSIZE* above 0.10 do not get disclosed as separate segments. The standard deviation of *RELSIZE* is 0.30 for disclosed pseudo-segments, which suggests that many with *RELSIZE* below 0.10 *do* get disclosed separately. Thus, *RELSIZE* is not deterministic for the pseudo-segment disclosure decision.

The results in Panels B and C of Table 2 reveal that the differences between the disclosing and non-disclosing pseudo-segments documented in Panel A also hold for the single-segment and multi-segment sample with one exception: for the single-segment sample, *PROFITADJ* is no longer statistically significantly larger for the non-disclosing pseudo-segments.

Table 3 includes a correlation matrix for the multi-segment sample with Pearson correlations above the diagonal and Spearman below.[15] Table 2 covers the main associations between our dependent variable, *MATCH*, and the treatment variables, so we do not repeat those associations here. We note from Table 3, however, that most of the explanatory variables have significant correlations not only with *MATCH*, but also with each other. We also note that *RELSIZE* has a large positive correlation with *MATCH* as well as statistically significant (although much smaller) correlations with the treatment variables. As we discuss in detail later, the relation between *RELSIZE* and *MATCH* also has an important nonlinearity due to the inclusion of a cutoff point for measures similar to *RELSIZE* in the accounting rules for segment reporting.

Base Model Logistic Regression Analysis

Full Sample

Table 4 presents the results of estimating our base model on the full, single-segment and multi-segment samples. The nine columns of numbers are divided into three sets of results for the three samples. For each, the first column of numbers presents coefficient estimates and the pseudo R^2 of the logistic regression. The pseudo R^2 exceeds 20 percent for all samples in Table 4 and in all of our subsequent logistic regressions, indicating reasonable goodness of fit. The middle column of numbers for each sample presents two-tailed p-values, and the third a measure of the economic magnitude of the variables' effects (the marginal probability effect of the variable with all other explanatory variables evaluated at their means, multiplied by one standard deviation of

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[14] More precisely, paragraph 15 of SFAS No. 14 requires an industry segment to be classified as a reportable segment if it satisfies one or more of the following tests when the tests are applied separately for each fiscal year for which financial statements are presented: (1) Its revenue is 10 percent or more of the combined revenue of all of the enterprise's industry segments. (2) The absolute amount of its operating profit or operating loss is 10 percent or more of the greater, in absolute amount, of: (i) The combined operating profit of all industry segments that did not incur an operating loss, or (ii) The combined operating loss of all industry segments that did incur an operating loss. (3) Its identifiable assets are 10 percent or more of the combined identifiable assets of all industry segments. SFAS No. 14 goes on in paragraph 16 to state that interperiod comparability could require an industry segment to (not) be reportable even if it falls below (above) the 10 percent cutoffs in the currently reported periods.

[15] For brevity, we present correlations only for the multi-segment sample because the majority of our multivariate analyses focus on this group.

TABLE 3

Correlation Matrix for Multi-Segment Firms

	MATCH	*I_PROFIT*	*PROFITADJ*	*PRIVATE*	*TRANSIN*	*LOENT_HISUB*	*LABOR*	*FSIZE*
MATCH		0.064	−0.064	−0.142	−0.054	0.011	−0.085	−0.224
		(<0.0001)	(<0.0001)	(<0.0001)	(0.000)	(0.432)	(<0.0001)	(<0.0001)
I_PROFIT	0.056		0.029	−0.017	0.003	−0.009	−0.008	−0.031
	(<0.0001)		(0.040)	(0.235)	(0.849)	(0.526)	(0.557)	(0.028)
PROFITADJ	−0.049	0.015		0.185	−0.016	−0.072	−0.068	0.032
	(0.001)	(0.277)		(<0.0001)	(0.261)	(<0.0001)	(<0.0001)	(0.025)
PRIVATE	−0.139	−0.024	0.152		0.038	0.109	0.175	−0.133
	(<0.0001)	(0.090)	(<0.0001)		(0.008)	(<0.0001)	(<0.0001)	(<0.0001)
TRANSIN	−0.054	0.003	−0.008	0.040		0.065	0.020	0.052
	(0.000)	(0.845)	(0.567)	(0.005)		(<0.0001)	(0.154)	(0.000)
LOENT_HISUB	0.011	−0.022	−0.048	0.117	0.065		−0.139	−0.005
	(0.432)	(0.118)	(0.001)	(<0.0001)	(<0.0001)		(<0.0001)	(0.708)
LABOR	−0.083	0.009	−0.116	0.202	0.026	−0.134		−0.172
	(<0.0001)	(0.533)	(<0.0001)	(<0.0001)	(0.066)	(<0.0001)		(<0.0001)
FSIZE	−0.225	−0.027	0.037	−0.134	0.050	−0.002	−0.160	
	(<0.0001)	(0.061)	(0.008)	(<0.0001)	(0.000)	(0.889)	(<0.0001)	
FOURFIRM	0.099	0.021	0.024	−0.687	0.018	0.012	−0.252	0.177
	(<0.0001)	(0.136)	(0.084)	(<0.0001)	(0.208)	(0.413)	(<0.0001)	(<0.0001)
RELSIZE	0.536	0.108	−0.037	−0.177	−0.046	0.009	−0.066	−0.379
	(<0.0001)	(<0.0001)	(0.009)	(<0.0001)	(0.001)	(0.530)	(<0.0001)	(<0.0001)
SEGDIVERSITY	0.219	0.008	−0.006	0.028	−0.030	−0.013	0.097	−0.616
	(<0.0001)	(0.579)	(0.670)	(0.049)	(0.037)	(0.343)	(<0.0001)	(<0.0001)
NUMEST	−0.342	0.008	0.022	−0.011	0.049	−0.004	0.035	0.675
	(<0.0001)	(0.573)	(0.128)	(0.418)	(0.001)	(0.752)	(0.014)	(<0.0001)
CEN_CMPSTAT	0.051	−0.010	−0.007	0.008	−0.034	0.041	−0.038	−0.224
	(0.000)	(0.460)	(0.640)	(0.563)	(0.015)	(0.004)	(0.007)	(<0.0001)
INDMATCHRATE	0.186	0.011	−0.102	−0.327	0.023	0.046	−0.140	0.014
	(<0.0001)	(0.422)	(<0.0001)	(<0.0001)	(0.105)	(0.001)	(<0.0001)	(0.319)

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	MATCH	*I_PROFIT*	*PROFITADJ*	*PRIVATE*	*TRANSIN*	*LOENT_HISUB*	*LABOR*	*FSIZE*
NEC	−0.064	−0.015	−0.044	0.114	−0.087	−0.133	0.091	−0.061
	(<0.0001)	(0.294)	(0.002)	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)

	FOURFIRM	*RELSIZE*	*SEGDIVERSITY*	*NUMEST*	*CEN_CMPSTAT*	*INDMATCHRATE*	*NEC*
MATCH	0.093	0.465	0.232	−0.295	0.047	0.182	−0.064
	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)	(0.001)	(<0.0001)	(<0.0001)
I_PROFIT	0.016	0.072	0.007	0.005	0.001	−0.001	−0.030
	(0.254)	(<0.0001)	(0.616)	(0.704)	(0.924)	(0.949)	(0.032)
PROFITADJ	−0.016	−0.039	−0.004	0.000	−0.006	−0.096	−0.014
	(0.274)	(0.006)	(0.766)	(0.995)	(0.683)	(<0.0001)	(0.329)
PRIVATE	−0.705	−0.192	0.041	−0.038	0.010	−0.310	0.108
	(<0.0001)	(<0.0001)	(0.003)	(0.008)	(0.469)	(<0.0001)	(<0.0001)
TRANSIN	0.012	−0.052	−0.028	0.066	−0.035	0.022	−0.087
	(0.389)	(0.000)	(0.050)	(<0.0001)	(0.015)	(0.123)	(<0.0001)
LOENT_HISUB	0.001	0.014	−0.013	0.012	0.042	0.048	−0.133
	(0.957)	(0.311)	(0.363)	(0.382)	(0.003)	(0.001)	(<0.0001)
LABOR	−0.290	−0.069	0.115	0.001	−0.042	−0.119	0.097
	(<0.0001)	(<0.0001)	(<0.0001)	(0.938)	(0.003)	(<0.0001)	(<0.0001)
FSIZE	0.172	−0.458	−0.616	0.567	−0.228	0.018	−0.066
	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)	(0.194)	(<0.0001)
FOURFIRM		0.102	−0.082	0.063	−0.016	0.176	−0.279
		(<0.0001)	(<0.0001)	(<0.0001)	(0.273)	(<0.0001)	(<0.0001)
RELSIZE	0.118		0.383	−0.381	0.075	0.161	−0.065
	(<0.0001)		(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)
SEGDIVERSITY	−0.097	0.383		−0.591	0.072	0.033	0.077
	(<0.0001)	(<0.0001)		(<0.0001)	(<0.0001)	(0.019)	(<0.0001)
NUMEST	0.052	−0.540	−0.654		−0.170	−0.049	−0.007
	(0.000)	(<0.0001)	(<0.0001)		(<0.0001)	(0.001)	(0.639)

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	FOURFIRM	*RELSIZE*	*SEGDIVERSITY*	*NUMEST*	*CEN_CMPSTAT*	*INDMATCHRATE*	*NEC*
CEN_CMPSTAT	−0.020 (0.166)	0.087 (<0.0001)	0.058 (<0.0001)	−0.167 (<0.0001)		−0.014 (0.318)	−0.031 (0.028)
INDMATCHRATE	0.182 (<0.0001)	0.148 (<0.0001)	0.031 (0.029)	−0.028 (0.052)	−0.010 (0.486)		−0.098 (<0.0001)
NEC	−0.314 (<0.0001)	−0.058 (<0.0001)	0.079 (<0.0001)	−0.011 (0.433)	−0.028 (0.046)	−0.107 (<0.0001)	

Correlations are estimated using 4,987 pseudo-segments from the years 1987, 1992, and 1997. The pseudo-segments all relate to firms that have multiple line-of-business segments per Compustat. Pearson (Spearman) correlations are above (below) the diagonal. p-values for two-tailed tests are shown in parentheses. Please refer to Table 1 for additional sample selection criteria and to Table 2 for variable definitions.

TABLE 4
Logistic Regression Estimates for Pseudo-Segment Disclosure Decision

		All Sample Firms			Single-Segment Firms Only			Multi-Segment Firms Only		
	Predicted Sign	**Coefficient**	**p-value**	**Economic Magnitude**	**Coefficient**	**p-value**	**Economic Magnitude**	**Coefficient**	**p-value**	**Economic Magnitude**
Intercept		−0.773	0.092		−1.040	0.078		−0.480	0.430	
Treatment Variables										
I_PROFIT	?	0.080	0.033	0.018	−0.020	0.716	−0.004	0.122	0.020	0.028
PROFITADJ	–	−0.139	0.128	−0.032	0.134	0.362	0.029	−0.314	0.005	−0.073
PRIVATE	–	−0.471	0.035	−0.108	−0.638	0.053	−0.140	−0.226	0.404	−0.052
TRANSIN	–	−0.157	0.004	−0.036	−0.061	−0.548	−0.013	−0.182	0.009	−0.042
LOENT_HISUB	–	0.050	0.493	0.011	0.086	0.480	0.019	−0.034	0.712	−0.008
LABOR	–	−0.675	0.171	−0.155	−0.904	−0.263	−0.198	−1.363	0.017	−0.316
Control Variables										
FSIZE		0.015	0.580	0.003	−0.091	0.023	−0.020	0.020	0.574	0.005
FOURFIRM		0.214	0.408	0.049	0.375	0.350	0.082	0.229	0.450	0.053
RELSIZE		4.073	<0.001	0.933	4.313	<0.001	0.946	5.816	<0.001	1.349
SEGDIVERSITY		0.039	0.838	−0.093	NA	NA	NA	−0.053	0.838	−0.012
NUMEST		−0.025	<0.001	−0.006	NA	NA	NA	−0.045	<0.001	−0.010
CEN_CMPSTAT		−0.407	0.178	−0.093	−0.325	0.404	−0.071	−0.207	0.550	−0.048
INDMATCHRATE		1.014	<0.001	0.232	0.779	<0.001	0.171	1.089	<0.001	0.253
NEC		−0.107	0.193	−0.024	−0.035	0.800	−0.008	−0.141	0.169	−0.032
Pseudo R^2		0.205			0.285			0.219		

The binary dependent variable is *MATCH*. All p-values are two-tailed. The "Economic Magnitude" column reflects the change in probability that the pseudo-segment is disclosed for a one standard deviation change in the variable (or an indicator variable that equals 1) with all other independent variables evaluated at their means. Please refer to Table 1 for additional sample selection criteria and to Table 2 for variable definitions. The regression depicted in the first three columns of results is estimated using 8,287 pseudo-segments from the years 1987, 1992, and 1997. The regression depicted in the middle three columns of results is estimated using 3,300 pseudo-segments from the years 1987, 1992, and 1997; the pseudo-segments all relate to firms that have a single line-of-business segment per Compustat. The regression depicted in the final three columns of results is estimated using 4,987 pseudo-segments from the years 1987, 1992, and 1997; the pseudo-segments all relate to firms that have multiple line-of-business segments per Compustat.

the variable, or by 1 if it is an indicator variable). We include multiple observations for the same firm in our regressions, so p-values are calculated using robust standard errors that correct for firm clustering.

The full sample results use both single-segment and multi-segment firms, but we argue these subsamples need to be evaluated separately. The full sample results are thus presented merely for completeness. We use them to discuss findings on the control variables, which are similar across all three samples. We defer discussion of the treatment variables for the separate single-segment and multi-segment subsample results.

The full sample results for several of the control variables are worth noting. First, the positive coefficient estimate on *RELSIZE* is consistent with pseudo-segments that represent a larger portion of their firm's sales being more likely to be disclosed. The estimate on *CEN_CMPSTAT* is not significantly different from zero. This indicates that including firm-years where Census *TVS* captures as little as 75 percent or as much as 125 percent of the total firm sales per Compustat does not distort our inferences. One variable that mitigates S&P measurement error, *INDMATCHRATE*, is highly significant, while the other, *NEC*, is not. The positive coefficient on *INDMATCHRATE* suggests that a pseudo-segment is more likely to be disclosed when other firms' pseudo-segments in the same industry tend to be disclosed. *NUMEST* captures the number of pseudo-segments at the firm level, and it is negatively associated with the likelihood of disclosing an individual pseudo-segment. This is intuitive, as more pseudo-segments increase the firm's ability to aggregate. Finally, the variable *FOURFIRM* is not related to *MATCH.*

We conclude from the control variable results that our dependent variable, *MATCH*, which captures the internal pseudo-segment disclosure aggregation decision, is reasonably associated with firm fundamentals that are likely to be non-strategic determinants of the disclosure decision.

Single-Segment Firms

The Census data are advantageous relative to Compustat data for analyzing aggregation decisions at single-segment firms. For example, Harris (1998) and Botosan and Harris (2000) evaluate only multi-segment firms. Berger and Hann (2007) and Botosan and Stanford (2005) examine firms that reported one segment under SFAS No. 14 that was restated to multiple segments under SFAS No. 131, but these restating firms represent a minority of all single-segment firms.

Single-segment firms likely differ from multi-segment firms in two related ways. First, from a strategic disclosure standpoint, line-of-business aggregation has fundamentally different consequences for single-segment versus multi-segment firms. If the firm chooses to report a single external segment, then outsiders know that *all* industries in which the firm operates are aggregated into that segment.[16] On the other hand, if the firm chooses to report multiple external segments, an additional layer of uncertainty is introduced. In this situation, there are now multiple reported divisions that might contain several possible combinations of aggregated (i.e., non-disclosed) business activities. Because this additional layer of strategy is not applicable to single-segment firms, we expect strategic factors to play a lesser role in their aggregation decisions. Second, we expect that agency costs play a smaller role in the aggregation decisions of single-segment firms because past research shows that multi-segment firms are traded at a discount relative to single-segment companies (Lang and Stulz 1994; Berger and Ofek 1995).

We present the results of our multivariate analyses of the single-segment sample in the middle three columns of numbers in Table 4.[17] The six treatment variables of interest are *I_PROFIT*,

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[16] Outsiders will not know about the profitability of individually aggregated activities, so some uncertainty remains.

[17] In this specification, we remove the control variables *SEGDIVERSITY* and *NUMEST* because we expect that once a firm

PROFITADJ, *PRIVATE*, *TRANSIN*, *LOENT_HISUB*, and *LABOR*. For the single-segment sample, only one of the treatment variables, *PRIVATE*, is associated with the likelihood of disclosure. Hence, there is no evidence that agency costs motivate the disclosure decisions of managers of single-segment firms. Rather, proprietary costs from private competition are the key strategic determinant of pseudo-segment aggregation for these firms.

Multi-Segment Firms

The last three columns of numbers in Table 4 present the results of estimating our base model on the multi-segment firms. The overall results are considerably different from those shown in either the first three columns of numbers (i.e., the full-sample results) or the middle three columns (i.e., the single-segment sample results). For the multi-segment sample, four of the treatment variables have a significant association with *MATCH*; moreover, *PRIVATE* is not one of these variables. In contrast, *PRIVATE* is the only treatment variable that is related to the aggregation choices of single-segment firms. These results suggest that compared to single-segment firms: (1) strategic motives play a larger role in the determination of multi-segment firms' aggregation choices and (2) agency cost motives for nondisclosure are more pertinent for multi-segment firms. These results also suggest that the proprietary costs that are relevant in the multi-segment firm context are different from those relevant to managers of single-segment firms.

Turning to individual results, *I_PROFIT* has a significantly positive coefficient estimate, implying that, on average, pseudo-segments with high abnormal profitability are more likely to be disclosed. The result is consistent with the agency cost hypothesis that pseudo-segments with low abnormal profitability are more likely to be aggregated by multi-segment firms. The economic magnitude of *I_PROFIT* is also significant; a one standard deviation increase in *I_PROFIT* increases the probability of pseudo-segment disclosure by 2.8 percent.

The *PROFITADJ* variable is significantly negative, suggesting pseudo-segments are more likely to be aggregated when they operate in industries with slow convergence of abnormal profits toward the industry mean (Harris 1998). The economic magnitude column shows that, for *PROFITADJ*, a one standard deviation increase from the mean value of 0.33 is associated with a 7.3 percent decrease in disclosure probability. Harris (1998) interprets this finding as consistent with the proprietary cost motive for aggregation. Such an interpretation implies that the slow convergence of the top performers toward the industry mean is driven by the stronger firms continuing to protect their proprietary advantages with less disaggregated disclosure. An alternative possibility is that the slow convergence of the top performers toward the industry mean can be driven by the weaker firms continuing to experience unresolved agency problems and failing to fully disclose their underperforming operations (Berger and Hann 2007). We explore these alternative explanations in subsequent analyses.

The significantly negative coefficient estimate of *TRANSIN* is consistent with the agency cost hypothesis that pseudo-segments are less likely to be disclosed when they receive inefficient transfers. Again, the economic magnitude is meaningful: when the pseudo-segment is classified as receiving inefficient transfers, the probability of nondisclosure increases by 4.2 percent.

The coefficient on *LOENT_HISUB* is insignificant, inconsistent with product-market competition as reflected in low barriers to entry and high product substitutability affecting segment disaggregation. Finally, we find a negative association between the importance of labor costs in the industry and the likelihood of pseudo-segments in that industry being separately disclosed. The significantly negative coefficient estimate on *LABOR* is highly important economically, with a one

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decides to report only one segment, the diversity and number of industries have no effect on the identity of the business activity that is reported as the firm's primary operation. None of our conclusions change when we include these variables in the model, and neither are significant when included.

standard deviation increase in labor costs associated with a 32 percent decrease in the probability of separately reporting the pseudo-segment.[18]

V. EXTENSIONS OF THE BASE MODEL AND SENSITIVITY ANALYSES

In this section we extend the base model estimated in Table 4. Given the sharp differences in results from estimating the base model on the multi-segment versus single-segment samples, it is clear that mixing these samples together clouds our inferences. Accordingly, we focus the remainder of the analysis on the multi-segment firms.

Nonlinear Size Effects

As discussed in footnote 14, the accounting rules for segment reporting suggest that the impact of relative pseudo-segment size on the aggregation decision is nonlinear. Specifically, the impact could depend on whether the size of the pseudo-segment exceeds the 10 percent cutoff mentioned in paragraph 15 of SFAS No. 14. Therefore, we modify the base model by adding seven new variables. In particular, we add an additional fixed effect, *SML_RELSIZE*, which is an indicator variable set equal to 1 when *RELSIZE* is less than 0.10 (i.e., a "small" pseudo-segment), and 0 otherwise (i.e., a "large" pseudo-segment). The remaining six new variables are interactions between *SML_RELSIZE* and our six treatment variables. After including these new variables, the coefficient on a particular treatment variable now reflects the relation between that variable and the likelihood that a large pseudo-segment is disclosed. The coefficients on the new interaction terms reflect how the relation varies between small and large pseudo-segments.

Table 5 reports the results from estimating this extended model.[19] Three main points emerge. First, the coefficient estimate on *SML_RELSIZE* is significantly negative, indicating that even after including the continuous variable *RELSIZE*, there remains a significant "size effect." Disclosure likelihood declines significantly when the pseudo-segment's relative size drops below the 10 percent cutoff specified by the accounting rules. Second, the coefficient estimates from the main effects of our six treatment variables are the same in Table 5 as for the multi-segment subsample in Table 4, with the exception that the estimate on *I_PROFIT* is no longer significant. Thus, pseudo-segments that have a relative size of at least 10 percent are generally affected by our treatment variables in the same manner inferred from the multi-segment results of Table 4.

Third, the coefficient estimates on the interactions between the *SML_RELSIZE* indicator and our six treatment variables are insignificant, with the exception of the estimate on *LABOR* × *SML_RELSIZE*, which is significant at the 0.10 level. We conclude that, while the disclosure decision is nonlinear with respect to pseudo-segment size (i.e., the main effect of *SML_RELSIZE* is significant), this nonlinearity does not have a first-order effect on our inferences (i.e., the interactions are almost all insignificant).

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[18] We also estimate this model on a sample of multi-segment firms that appear in all three years of the panel. This sample consists of 2,281 pseudo-segments, or approximately 46 percent of the original sample. Our inferences are unchanged when using this specification, with the exceptions that while *I_PROFIT* and *LABOR* continue to have the same signs, the statistical significance of the estimated coefficients declines to p-values of 0.21 and 0.47, respectively.

[19] Research by Ai and Norton (2003) and Powers (2005) emphasizes that it can be potentially difficult to interpret interaction terms from nonlinear models such as probit or logistic regression models *if* one is interested in assessing marginal effects at a point other than the center of the distribution and *if* the researcher is interested in quantifying the marginal effect of the interaction term on the probability of an event (pseudo-segment disclosure in our setting). We do not believe that either of these circumstances applies to our Table 5 regression nor do they apply to the subsequent untabulated sensitivity tests that use the same interactions as in Table 5 (between *SML_RELSIZE* and our six treatment variables). Nevertheless, from an abundance of caution we further ensure that our inferences are correct by adopting the methodology developed by Norton et al. (2004) to compute marginal effects of the interaction terms and assessing these effects over the entire range of predicted probabilities from our logistic regression models. We find that our inferences are unambiguous across the entire range of predicted probabilities for all of the models that contain interactive terms.

TABLE 5

Logistic Regression Estimates for Pseudo-Segment Disclosure Decision of Multi-Segment Firms Interactions between Treatment Variables and *SML_RELSIZE* are Included

	Predicted Sign	Coefficient	p-value	Economic Magnitude
Intercept		0.605	0.347	
SML_RELSIZE		−1.558	<0.001	−0.359
Treatment Variables				
I_PROFIT	?	0.073	0.489	0.017
I_PROFIT × *SML_RELSIZE*	?	0.051	0.676	0.012
PROFITADJ	−	−0.459	0.020	−0.105
PROFITADJ × *SML_RELSIZE*	?	0.230	0.323	0.052
PRIVATE	−	−0.202	0.560	−0.046
PRIVATE × *SML_RELSIZE*	?	0.107	0.810	0.024
TRANSIN	−	−0.238	0.042	−0.053
TRANSIN × *SML_RELSIZE*	?	0.089	0.558	0.020
LOENT_HISUB	−	0.025	0.868	0.006
LOENT_HISUB × *SML_RELSIZE*	?	−0.120	0.512	−0.027
LABOR	−	−2.723	0.005	−0.620
LABOR × *SML_RELSIZE*	?	2.109	0.061	0.480
Control Variables				
FSIZE		0.035	0.346	0.008
FOURFIRM		0.245	0.418	0.056
RELSIZE		3.350	<0.001	0.763
SEGDIVERSITY		−0.008	0.975	−0.002
NUMEST		−0.043	<0.001	−0.010
CEN_CMPSTAT		−0.224	0.524	−0.051
INDMATCHRATE		1.083	<0.001	0.247
NEC		−0.140	0.181	−0.031
Pseudo R^2		0.233		

The regression is estimated using 4,987 pseudo-segments from the years 1987, 1992, and 1997. The pseudo-segments all relate to firms that have multiple line-of-business segments per Compustat. The binary dependent variable is *MATCH*. *SML_RELSIZE* is an indicator variable that equals 1 (0) if *RELSIZE* is (not) less than 0.10. All p-values are two-tailed. The "Economic Magnitude" column reflects the change in probability that the pseudo-segment is disclosed for a one standard deviation change in the variable (or an indicator variable that equals 1) with all other independent variables evaluated at their means. Please refer to Table 1 for additional sample selection criteria and to Table 2 for variable definitions.

Effects of the Excess Value of Diversification

Our results suggest that the motive and opportunity to aggregate for agency cost reasons are greater for multi-segment firms than for single-segment firms. Within the multi-segment sample, however, there is likely considerable variation in the agency cost motive. To exploit this variation, we conduct additional tests by re-estimating our base regression on two subsamples of the multi-segment sample. In particular, we separate pseudo-segments into two groups based on whether the pseudo-segment belongs to a firm with negative or nonnegative estimated excess-value from diversification. We use the methodology discussed in Berger and Ofek (1995) to estimate excess values from diversification. A negative excess value from diversification indicates that the value of the multi-segment firm is lower than the value of a collection of stand-alone firms consisting of the multi-segment firm's separate lines of business. To the extent this value loss is a manifestation of agency problems, negative excess value firms are more likely to have agency costs than firms with nonnegative excess values.

Table 6 shows the results from estimating our base model on the subsamples of multi-segment firms that have negative and nonnegative excess values. Of the 4,987 pseudo-segments available for the logistic regressions in Tables 4 and 5, the requisite data for calculating excess values are available for 4,517. Of these, 2,545 (56 percent) pseudo-segments are from firm-years with negative excess values, and the remaining 1,972 (44 percent) pseudo-segments are from firm-years with nonnegative excess values. Overall, for all multi-segment firms, untabulated results show that the mean excess value is −0.13 and the median is −0.08, suggesting that the diversification discount identified in past literature on broad samples (Berger and Ofek 1995; Lang and Stulz 1994) exists in our sample of firms concentrated in domestic manufacturing.

TABLE 6

Logistic Regression Estimates for Pseudo-Segment Disclosure Decision of Multi-Segment Firms Separate Regressions Estimated for Firms with Negative and Nonnegative Excess Values from Diversification

		Negative Excess Value Subsample			Nonnegative Excess Value Subsample		
	Predicted Sign	Coefficient	p-value	Economic Magnitude	Coefficient	p-value	Economic Magnitude
Intercept		−0.375	0.567		−2.126	0.048	
Treatment Variables							
I_PROFIT	?	0.153	0.028	0.037	0.117	0.124	0.026
PROFITADJ	−	−0.229	0.135	−0.055	−0.399	0.007	−0.088
PRIVATE	−	−0.302	0.373	−0.072	0.113	0.794	0.025
TRANSIN	−	−0.175	0.091	−0.041	−0.201	0.087	−0.044
LOENT_HISUB	−	−0.009	0.936	−0.002	−0.026	0.873	−0.006
LABOR	−	−2.360	0.001	−0.563	0.313	0.733	0.069
Control Variables							
FSIZE		0.030	0.425	0.007	0.065	0.332	0.014
FOURFIRM		0.567	0.126	0.135	−0.091	0.858	−0.020
RELSIZE		5.142	<0.001	1.226	7.028	<0.001	1.555
SEGDIVERSITY		0.299	0.316	0.071	−0.283	0.531	−0.063
NUMEST		−0.041	<0.001	−0.010	−0.046	<0.001	−0.010
CEN_CMPSTAT		−0.402	0.290	−0.096	0.678	0.239	0.150
INDMATCHRATE		1.056	<0.001	0.252	1.266	<0.001	0.280
NEC		−0.194	0.152	−0.046	−0.042	0.779	−0.009
Pseudo R^2		0.204			0.239		

The regressions are estimated using 4,517 pseudo-segments from the years 1987, 1992, and 1997. The pseudo-segments all relate to firms that have multiple line-of-business segments per Compustat. The 2,545 pseudo-segments underlying the regression in the first three columns of results relate to firm-years in which the excess value of diversification as measured using the sales-multiple approach discussed in Berger and Ofek (1995) is negative. The 1,972 pseudo-segments underlying the regression in the second three columns of results relate to firm-years in which the excess value of diversification is non-negative. The binary dependent variable is *MATCH*. All p-values are two-tailed. The "Economic Magnitude" column reflects the change in probability that the pseudo-segment is disclosed for a one standard deviation change in the variable (or an indicator variable that equals 1) with all other independent variables evaluated at their means. Please refer to Table 1 for additional sample selection criteria and to Table 2 for variable definitions.

With regard to the proprietary cost variables, neither *PRIVATE* nor *LOENT_HISUB* are associated with *MATCH* in either the negative excess value subsample (first three columns of numbers of Table 6) or the nonnegative excess value subsample (last three columns of numbers). Hence, we continue to find no evidence that either of these factors affect multi-segment firms' pseudo-segment aggregation decisions. The results for *PROFITADJ* do, however, offer support for the proprietary cost hypothesis because the significantly negative estimate on this variable that we observe in Tables 4 and 5 is attributable to the subsample of multi-segment firms that have nonnegative excess values. Given that positive excess value firms presumably do not have an agency cost motive to suppress disclosure, this finding is consistent with *PROFITADJ* capturing a proprietary cost motive for pseudo-segment aggregation.

Turning to the agency cost variables, the estimates on *I_PROFIT* and *TRANSIN* are significant in the predicted direction for the negative excess value subsample. Thus, as expected, firms with value-reducing diversification strategies, and arguably higher agency costs, are less likely to separately disclose pseudo-segments that are less profitable, as well as those that receive more inefficient transfers. However, as shown in the final three columns of numbers of Table 6, the coefficient on *TRANSIN* is also significantly negative for the nonnegative excess value subsample. Moreover, the proprietary cost variable *LABOR* is significant in the negative excess value subsample, but not the nonnegative excess value subsample, counter to expectations. Thus, while the agency cost motive for aggregating pseudo-segments appears to apply strongly to the multi-segment firms with unsuccessful diversification strategies, there are some inconsistencies.[20]

Untabulated Sensitivity Analyses

Validation and Sensitivity Testing of our Dependent Variable, MATCH

As discussed earlier, we face a difficult research design issue in identifying the aggregation schemes firms *should* use if they neutrally apply the accounting principles that mandate separate industry disclosure. The accounting standard in effect for the sample period (SFAS No. 14) explicitly rejects applying any single industry-classification system to all firms yet, in order to conduct an objective study, we are forced to adopt a single system. In this section we provide empirical evidence supporting our earlier arguments and perform sensitivity analyses in which we use an alternative aggregation scheme based on three-digit SIC codes.

We begin by evaluating the construct validity of our disclosure measure, *MATCH*, using the data from Berger and Hann (2003, 2007), who study firms that restate historical SFAS No. 14 segment data as part of their initial adoption of SFAS No. 131. There are 99 firms that overlap between our sample and theirs.[21] We calculate two ratios at the firm level, one from each data set. First, using Berger and Hann's data, we identify the newly revealed segments in 1998 annual reports; these are segments that were not disclosed initially in 1997 under SFAS No. 14, but were subsequently revealed as restated data upon initial adoption of SFAS No. 131. For each firm, we divide these newly revealed segments by the total number of restated 1997 segments per the 1998

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[20] We evaluate the robustness of the Table 6 results by estimating the extended version of the base model that incorporates *SML_RELSIZE* and its interactions with our six treatment variables. The (untabulated) results show that the only finding for the negative excess value sample sensitive to using the expanded model is that the coefficient on *I_PROFIT* is no longer significant. For the nonnegative excess value sample, two results are noteworthy. First, the marginally significant, and negative, coefficient on *TRANSIN* found in Panel B is statistically insignificant in the expanded model. Thus, the somewhat surprising finding that receiving inefficient transfers reduces the likelihood of pseudo-segment disclosure at firms with value-enhancing diversification programs is not robust. Second, similar to the results shown in Table 5, the coefficient estimate on *LABOR×SML_RELSIZE* is positive and significant.

[21] We analyze 1,625 firm years across 1987, 1992, and 1997. Berger and Hann (2003, 2007) use 796 restating firms across 1997 and 1998. The overlap is reduced from 796 because we require our firms to be almost exclusively involved in *domestic manufacturing*, whereas Berger and Hann have neither the industry nor geographic limitation *and* they sample from 1998, which we do not.

annual report. Second, using Census data, we identify the total number of unmatched pseudo-segments (i.e., *MATCH* equals 0) for each firm in 1997, and divide this by the total count of pseudo-segments for that firm-year.

Both of the ratios described above are measures of nondisclosure, and should be positively correlated. Empirically, we find that this is the case, with a Pearson correlation of 0.25 (p-value of 0.01) and a Spearman correlation of 0.22 (p-value of 0.03). Hence, our dependent variable *MATCH* exhibits external validity.

We next discuss results from using three-digit SIC codes to define industry in lieu of four-digit SIC codes. We do not believe this approach is appropriate for our primary analyses, as it adopts a disclosure benchmark that is more aggregated. If financial statement users demand detailed industry information, then a classification scheme that aggregates more and more disparate types of operations is contradictory to their demands.[22] Ultimately, this is a subjective research design choice on our part, and our conclusions based on four-digit aggregation schemes are valid only to the extent one believes, as we do, that this is a reasonable level of aggregation.

We repeat our logistic regressions for the multi-segment sample after adjusting the Table 4 specification in three ways. First, we form pseudo-segments on the basis of three-digit SIC codes and use a revised version of *MATCH* equal to 1 (0) if the pseudo-segment matches (does not match) either the primary or secondary three-digit SIC code of any of the firm's externally reported segments. Second, we define industries on the basis of three-digit SIC codes for all of our industry-adjusted (i.e., *I_PROFIT* and *TRANSIN*) and industry-level (e.g., *PROFITADJ* and *PRIVATE*) variables. Finally, we exclude *NEC*, which is designed to control for measurement error related to miscellaneous industries and/or industries that are difficult to classify. These issues are irrelevant when moving to the more aggregated three-digit SIC code level.

The (untabulated) results from the specification described above show that the treatment variables *PROFITADJ* and *TRANSIN* have strong negative and highly significant (p-values < 0.01) associations with *MATCH* even when industry definitions are based on three-digit SIC codes. Hence, the main results documented in the tables are robust to an alternative industry classification scheme. However, there are two pertinent differences between the untabulated results and the results presented in Table 4 for multi-segment firms: (1) the treatment variables *I_PROFIT* and *LABOR* are no longer significant and (2) the control variable *FOURFIRM* has a positive, significant association with *MATCH*.

Effects of Information Environment Variables

As a final robustness test, we add four explanatory variables to our regressions that capture aspects of the information environment associated with disclosure quality: (1) the average number of I/B/E/S analysts covering the firm over the fiscal year; (2) return on sales; (3) a flow-of-funds based measure of the firm's net debt issuance, measured following Berger et al. (1997); and (4) a flow-of-funds based measure of new equity issued by the firm scaled by total assets [i.e., Compustat annual data items (#108)/(#6)].

The (untabulated) results from estimating this extended version of the Table 4 specification lead to the same inferences as those drawn from each subsample in Table 4. We also estimate an extended version of the Table 5 model that includes each of the four information environment variables described above. The (untabulated) results from this regression lead to the same inferences as those drawn from the results shown in Table 5, except that the coefficient estimate on *LABOR* × *SML_RELSIZE* is no longer significantly different from zero.

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[22] The development of the more detailed NAICS suggests that even the four-digit SIC code system may be too coarse for users of industry data.

VI. CONCLUSION

We examine discretionary disclosure with unique data by examining a comprehensive set of forces hypothesized to influence segment reporting decisions. Our data consist of *confidential* information about internal operating results for a large sample of U.S. manufacturing firms, thereby capturing various aspects of agency and proprietary cost motives for nondisclosure. The combination of unique data and a broad set of disclosure determinants allows us to provide new insights regarding the motives underlying managers' aggregation decisions.

We provide evidence of differences between the disclosure behavior of single-segment firms and multi-segment firms. Single-segment firms exhibit less strategic behavior and, to the extent these firms strategize, proprietary costs related to private competition appear to be the key motive for nondisclosure. On the other hand, both proprietary and agency cost motives are important determinants of multi-segment firms' segment disclosure decisions.

One influence of agency costs on the disclosure choices of the multi-segment firms in our sample is that, if a pseudo-segment is receiving inefficient transfers of capital from the rest of the firm, it is less likely to be disclosed separately. This result is robust to alternative specifications. We also show that pseudo-segments with low industry-adjusted profits are more likely to be aggregated into another external segment and, thus, hidden from outsiders. However, this result is sensitive to model specification.

With respect to proprietary costs, we find that if a multi-segment firm has operations in an industry in which positive abnormal profits of some industry members (relative to the industry average) are likely to persist, it is less likely to disclose those operations separately. This result is robust and is manifest primarily in the subsample of firms least likely to have an agency cost motive for nondisclosure (i.e., firms for which diversification appears to be value-enhancing). This suggests a proprietary cost effect, consistent with conclusions drawn by Harris (1998). We also find that multi-segment firms are more likely to aggregate operations that are from an industry with greater labor power, although this result is sensitive to model specification.

Finally, in addition to our evidence on the proprietary and agency cost motives for aggregation, we show that comprehensive measures of industry statistics (i.e., including both privately held and publicly traded firms) are useful for measuring industry-adjusted and industry-level disclosure determinants.

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The Effect of Trading Volume on Analysts' Forecast Bias

Anne Beyer
Ilan Guttman
Stanford University

ABSTRACT: This study models the interaction between a sell-side analyst and risk-averse investors. It derives an analyst's optimal earnings forecast and investors' optimal trading decisions in a setting where the analyst's payoff depends on the trading volume the forecast generates as well as on the forecast error. In the fully separating equilibrium, we find that the analyst biases the forecast upward (downward) if his private signal reveals relatively good (bad) news.

The model predicts that: (1) the analyst biases the forecast upward more often than downward and the forecast is on average optimistic; (2) the magnitude of the analyst's bias is increasing in the per-share benefit from trading volume he receives; and (3) the analyst's expected squared forecast error may increase in the precision of his private information. Finally, we characterize the circumstances under which the (rational) analyst acts as if he overweights or underweights his private information.

Keywords: *analysts' forecasts; forecast bias; trading volume; trading commission.*

I. INTRODUCTION

Sell-side analysts generate revenue for brokerage firms via trading commissions. To provide appropriate incentives, brokerage firms often tie analysts' incentives to measures of trading volume in the stocks they cover. The use of trading volume in analysts' incentive schemes is supported by Cowen et al. (2006, 125) who report that "brokerage firms usually reward their research analysts using a single measure of performance: trading volume in the stocks they cover." While trading incentives have always been part of analysts' compensation formulas, recent regulatory changes that prohibit linking analysts' compensation to investment banking activities have magnified the importance of trading incentives for analysts.

When issuing a forecast, contemporary analysts' incentives are not limited to generating trading commissions. Instead, analysts also consider their ability to generate trade in the future. As

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Jackson (2005, 674) states, "[t]he analyst must trade off the short-term incentive to lie and generate more trade against the long-term gains from building a good reputation." Generally, analysts are concerned about the accuracy of their forecasts because errors in forecasts can adversely affect reputation, reduce compensation (Mikhail et al. 1999), affect rankings among analysts (Stickel 1992), and call into question whether the analysts have fulfilled their fiduciary responsibility to investors (Morgan and Stocken 2003).[1]

Although forecast errors are costly to analysts, they may still choose to bias their forecasts. Prior literature has documented that analysts do in fact bias their forecasts (e.g., Dugar and Nathan 1995; Michaely and Womack 1999; Kothari 2001; Lim 2001; Hong and Kubik 2003; Jackson 2005; Chen and Jiang 2006; Bernhardt et al. 2006). While the literature studies various reasons for analysts' bias, several studies provide evidence that analysts' forecast bias is driven by trading commissions (e.g., Jackson 2005; Cowen et al. 2006).

Our objective is to develop a model of an analyst's forecasting strategy in a setting in which the analyst's payoff depends on the trading volume his forecast generates as well as on his forecast error.[2] In addition to solving for the equilibrium of the model, we derive testable empirical predictions for the bias in the analyst's forecast and its effect on the analyst's expected forecast error. While several of the model's empirical predictions are supported by existing empirical studies, others, to the best of our knowledge, have not yet been tested.

In our model, the analyst benefits from the trading volume of investors who receive his forecast (informed investors). When issuing the forecast, the analyst is not confined to tell the truth but may choose to issue a forecast that differs from his expectations. We assume that forecast errors are costly to the analyst and that the analyst trades off costs from forecast errors against incentives to generate trading volume. This trade-off determines the equilibrium properties of the analyst's forecasting strategy. In the analysis, we focus on fully separating equilibria in which investors who observe the analyst's forecast can perfectly infer the analyst's bias and private information. In a more realistic setting, one might expect that this is not the case. Our equilibrium is robust to the introduction of additional information asymmetry that prevents investors from perfectly inferring the analyst's private information (Fischer and Verrecchia 2000; Dye and Sridhar 2004).

We show that the analyst's bias is increasing in his private signal and bounded from both below and above. If the analyst's signal is sufficiently unfavorable, such that the analyst expects informed investors to sell shares, then he biases the forecast downward. Otherwise, the analyst expects informed investors to buy additional shares and biases the forecast upward. While the analyst biases the forecast both upward and downward, depending on his private signal, we show that the analyst biases the forecast upward more often than downward—resulting in the analyst's forecast being optimistic on average. This is consistent with the majority of empirical studies on analysts' bias (for a review see Kothari [2001]).

In the model, the analyst biases the forecast upward more often than downward because of investors' risk aversion. Since both informed and uninformed investors are risk-averse, their demand for the firm's shares depends on the residual uncertainty they are exposed to. The analyst's forecast decreases the uncertainty to which both informed and uninformed investors are exposed;

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[1] Analysts' earnings forecast accuracy is a major factor in StarMine's evaluation of research analysts. Many Wall Street firms, including several of the ten that were involved in the Global Settlement, use StarMine data in determining their analysts' payment. Analysts' incentives may also be tied to their firms' performance through other channels such as equity compensation, bonus related to firm performance, and career concerns.

[2] For simplicity, we use male pronouns to refer to the analyst, but the gender is arbitrary; the reader should interpret each instance of "his" and "he" as meaning "his or her" and "he or she," respectively. For balance, we later arbitrarily refer to a single informed investor using the female pronouns "her" and "she."

however, the residual uncertainty of the analyst's clients (i.e., informed investors) is lower than the residual uncertainty of uniformed investors. The reason is that the information in the analyst's forecast is only partially reflected in stock prices, such that uninformed investors cannot perfectly infer the analyst's private signal. Since informed investors are exposed to less residual uncertainty than uninformed investors, the per-capita demand of informed investors for the firm's shares is on average higher than uninformed investors' per-capita demand. Therefore, the signal following which informed investors do not trade is, on average, a signal that conveys negative news. When informed investors do not trade, the analyst has no incentive to bias the forecast. Hence, in equilibrium the analyst does not bias the forecast only if his private signal equals the negative news for which informed investors do not trade on average. For more (less) favorable signals, the analyst biases the forecast upward (downward). As a result, the analyst biases the forecast upward more often than downward.

The model offers the following additional predictions. First, an increase in the analyst's per-share benefit from trading volume increases both the magnitude of the bias for any realization of the analyst's private signal and the expected forecast bias. As the analyst's per-share benefit from trading volume converges to zero, the bias converges to zero as well. This is consistent with Chen and Jiang (2006), who find that the deviation of the analyst's forecast from unbiased rational expectations increases when the analyst's benefits from doing so are high and when the costs of doing so are low.

Another prediction of the model relates the analyst's expected squared forecast error to the precision of his private information. One might expect that analysts who obtain a signal of higher precision issue a forecast that results in a smaller expected squared forecast error (distribution effect). However, the model shows that this does not have to be the case in equilibrium. Instead, the analyst's expected squared forecast error may increase in the precision of the analyst's private information. The reason is that, in addition to the distribution effect, an increase in the precision of the analyst's private signal also affects the analyst's incentives to bias the forecast. In particular, a higher precision of the analyst's private signal increases the sensitivity of informed investors' demand to the information conveyed in the analyst's forecast. This increased sensitivity boosts the analyst's incentive to bias the forecast (incentive effect). To summarize, the model predicts that analysts with higher precision of private information do not necessarily issue forecasts that result in smaller expected squared forecast errors. This result may shed some light on the surprising empirical findings that analysts who are conjectured to possess more precise information about a firm do not always outperform analysts with less precise information in terms of forecast errors (e.g., Gu and Xue 2008, in the context of affiliated versus independent analysts).

Finally, if the analyst's private signal conveys either positive or sufficiently negative news, then the analyst acts as if he overweights his private information—i.e., the analyst issues a forecast as if he places a larger weight on his private information than if he followed Bayes' Rule and truthfully reported his posterior expectation. If the analyst's private signal conveys moderately negative news, then the analyst issues a forecast as if he underweights his private information. In line with these predictions, Chen and Jiang (2006) provide evidence that analysts who issue forecasts that exceed the consensus overweight their private information. If they issue forecasts that are lower than the consensus, then analysts overweight their private information to a lesser extent and sometimes even underweight it. Easterwood and Nutt (1999), Friesen and Weller (2006), and Bernhardt et al. (2006) find that analysts do not act as if they rationally update their beliefs and truthfully report their expectations. Consistent with our model's prediction, all three studies find that analysts act as if they overweight positive private information. The empirical evidence regarding whether analysts act as if they over- or underweight negative private information is mixed. Since our model predicts that analysts may act as if they either over- or underweight negative private information, depending on the magnitude of the negative news, our model may suggest a way to reconcile the mixed empirical evidence.

We study the analyst's forecasting decision in a setting similar to Grossman and Stiglitz (1980) wherein the analyst provides his forecast to a fraction of strategic investors. In addition to investors who observe the analyst's forecast, there are two other groups of investors: investors who are strategic in their trade but do not observe the analyst's forecast and noise traders who trade in the risky asset in a non-strategic way. This model setup guarantees that the firm's share price reflects the analyst's forecasting behavior but does not fully reveal the analyst's private information. While this market setting captures various realistic features, for example that the analyst cannot perfectly predict the trading volume that his forecast will generate, it is not designed to accommodate per-share trading costs.[3] Per-share trading costs that investors pay is an important aspect of capital markets. To study the effect of per-share trading costs that investors face, we study an alternative setting in which the analyst provides his forecast to a single investor who is small enough to act as a price-taker. This alternative setting allows us to analyze the effect of constant per-share trading costs. We find that the analyst's forecasting behavior is robust to this change in assumption and that the empirical predictions that we discussed above continue to hold.

In terms of related theoretical literature, our model is most closely related to Hayes (1998). While our model focuses on how analysts' incentives to generate trading volume affect their forecasting decision, Hayes (1998) studies how incentives to generate trading commissions affect analysts' decisions to cover stocks and gather information.[4] In contrast to Hayes (1998), we assume that the precision of the analyst's information is exogenously given, but we allow the analyst to intentionally bias the forecast. That is, the analyst may issue a forecast that deviates from his own expectation about the firm's future performance. Other related theoretical work includes Stocken (2000) and Fischer and Stocken (2010), who study the amount of information conveyed and produced by analysts in cheap-talk settings; Lim (2001) and Beyer (2008), who study analysts' forecast bias when they incur costs from forecast errors; and Guttman (2010), who endogenizes the timing of analysts' forecasts in the presence of competition between analysts. In contrast to Hayes (1998) and our model, these other models do not consider how analysts' incentives to generate trading volume affect their behavior.

II. SETUP

We model an analyst's forecasting decision when his incentives are tied to the trading volume his forecast generates. We adopt the setting of Grossman and Stiglitz (1980), where the analyst provides his forecast to a fraction $\lambda \in (0,1)$ of strategic investors ("informed investors").[5] In addition to the informed investors who receive the analyst's forecast, there are two additional groups of investors: $(1-\lambda)$ uninformed investors who are strategic in their trade but do not observe the analyst's forecast, and noise traders who trade in the risky asset in a non-strategic way. All strategic investors can allocate their wealth across a risk-free asset and a single risky asset and exhibit homogenous CARA preferences given by $-e^{-\rho W_2}$, where W_2 is the final wealth and ρ is the risk-aversion coefficient.[6] As in Grossman and Stiglitz (1980), all strategic investors are price-takers, although in aggregate their demand affects the market price.

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[3] In order to introduce trading costs in the Grossman-Stiglitz setting and maintain the linearity of the equilibrium, we have to assume that the trading costs are quadratic. Under this assumption, the results of the model described above remain qualitatively unchanged. We pursue a different strategy in Section V of the study, since in practice marginal trading costs are unlikely to be increasing.

[4] Irvine (2004) tests empirical predictions based on Hayes (1998) and also examines how investors' trading decisions relate to the absolute value of analysts' *ex post* forecast errors.

[5] We take the fraction of informed investors as exogenous. However, the market for information is in equilibrium if informed investors have to pay the same fixed cost to obtain the private information as in Grossman and Stiglitz (1980).

The market for the risky asset opens twice—once before the analyst provides the forecast to the informed investors, at $t = 0$, and once after, at $t = 1$. Grossman and Stiglitz (1980) focus on investors' equilibrium holdings at time $t = 1$. We analyze investors' equilibrium holdings not only at time $t = 1$, but also at $t = 0$ in order to derive the informed investors' trading volume at $t = 1$, which determines the analyst's payoff.

After each round of trade, the sum of the holdings of the informed investors, uninformed investors, and noise traders has to add up to 1. We assume that noise traders' holdings equal $(1 - \mu_s)$ at $t = 0$ and $(1 - s)$ at $t = 1$, respectively. The market-clearing conditions for the firm's shares at $t = 0$ and $t = 1$, respectively, are:

$$\lambda D_0^I + (1 - \lambda) D_0^U = \mu_s; \tag{1}$$

$$\lambda D_1^I + (1 - \lambda) D_1^U = s, \tag{2}$$

where D_t^I (D_t^U) denotes the per-capita holdings of an informed (uninformed) investor at time $t =$ 0,1. P_t denotes the market clearing price at $t =$ 0,1.

We assume that noise traders' supply at $t = 1$, s, is drawn from a normal distribution with mean μ_s and variance σ_s^2.[7] Noise traders' supply at $t = 1$ is random and, hence, the equilibrium price does not fully reveal informed investors' private information. At $t = 0$, no strategic investor has private information and, hence, no random supply is needed to prevent the market price from fully revealing such information.[8] The assumption that $E[\tilde{s}] = \mu_s$ implies that the combined holdings of strategic investors at $t = 1$ are on average the same as at $t = 0$.[9] We assume $\mu_s > 0$ to reflect that strategic investors as a group hold a long-position in the firm at $t = 0$ and hold a long-position on average at $t = 1$.

The realized value of the firm's earnings, x, are not directly observable to anyone outside the firm. However, an analyst obtains private information about the firm's earnings. The analyst's private information, Ψ, is a noisy signal of firm's earnings. In particular, $\tilde{x} = \tilde{\psi} + \tilde{\varepsilon}$, where $\tilde{\psi}$ and $\tilde{\varepsilon}$ are independently and normally distributed according to $\tilde{\psi} \sim N\left(\mu_x, \frac{1}{\tau_\psi}\right)$ and $\tilde{\varepsilon} \sim N\left(0, \frac{1}{\tau_\varepsilon}\right)$, respectively. This implies that the firm's earnings are distributed normally with mean μ_x and precision $\tau_x = \frac{\tau_\psi \tau_\varepsilon}{\tau_\psi + \tau_\varepsilon}$. Based on his private information, the analyst provides a forecast, x^R, of the firm's future earnings to his clients, the informed investors.

When issuing the forecast, the analyst is not confined to tell the truth. Rather, the analyst can release a forecast that differs from his own beliefs about the firm's expected earnings given his private information Ψ. The analyst may want to bias the forecast in order to increase the expected trading volume generated by informed investors following the analyst's forecast.[10] However, the analyst incurs a personal cost from forecast errors where forecast errors measure the difference between the forecast and the realization of the firm's earnings. When deciding what forecast to issue, the analyst trades off the expected cost from forecast errors against the expected benefits from increased trading volume. The analyst's costs are assumed to be quadratic in the forecast error.

---

[6] While we assume the same risk-aversion coefficient for all strategic investors, the characteristics of the equilibrium remain qualitatively unchanged if the risk-aversion coefficient of informed investors, $\rho_I > 0$, differs from that of uninformed investors, $\rho_U > 0$. Proofs are available upon request.

[7] We follow Grossman and Stiglitz (1980) and label s as the noise traders' supply of the risky asset (even though $(1 - s)$ refers to noise traders' holdings at $t = 1$).

[8] We may also allow the noise traders' supply at $t = 0$ to be random. For instance, suppose the noise traders' supply at $t = 0$, s_0, is distributed over (0,1) and noise traders' supply at $t = 1$ is normally distributed with mean s_0. As long as either the noise traders' supply s_0 or strategic investors' holdings at $t = 0$ are revealed before the analyst issues his forecast, the equilibrium remains qualitatively unchanged.

[9] We could also allow for $E[\tilde{s}] > \mu_s$ without qualitatively affecting the empirical predictions. If $E[\tilde{s}] < \mu_s$, then the empirical predictions remain qualitatively the same as long as $E[\tilde{s}]$ is sufficiently close to μ_s. If $E[\tilde{s}] \ll \mu_s$, then the characteristics of the analyst's bias function remain intact except for the median bias being negative instead of positive. As a result, some empirical predictions change. For a more detailed discussion of the effect of μ_s on the analyst's reporting strategy, please see below.

We assume that the analyst benefits from the trading volume his forecast generates among informed investors. Informed investors' trading volume is the absolute value of the difference between their initial holdings, λD_0^I, and their holdings after observing the analyst's forecast, λD_1^I. The noisy supply prevents the analyst from perfectly anticipating both the price P_1 and the informed investors' trade $D_1^I(x^R, P_1) - D_0^I$. Therefore, the analyst maximizes his expected benefit from trading volume net of his expected costs from forecast errors. We denote the analyst's marginal benefit from trading volume by c_A. To summarize, the analyst chooses his forecast, x^R, that maximizes his expected utility given his private information Ψ:

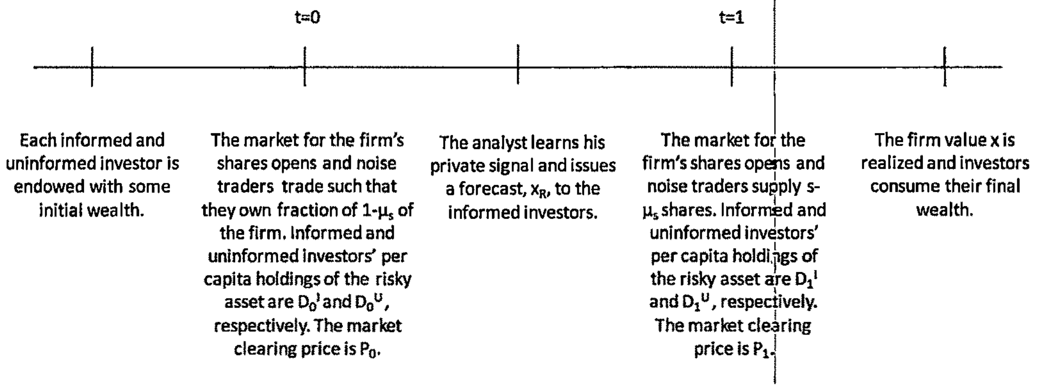
$$x^R(\psi) \in \arg\max_{x^R} c_A E[\lambda|D_1^I(\psi,\tilde{P}_1) - D_0^I||\psi] - \frac{1}{2}E[(x^R - \tilde{x})^2|\psi]. \tag{3}$$

All parameters and the structure of the model are assumed to be common knowledge. Figure 1 summarizes the timeline of the model.

III. EQUILIBRIUM

We solve for the Perfect Bayesian Equilibrium of the game. In equilibrium, each strategic investor maximizes the expected utility of final consumption, conditional on all the information available to her including current equilibrium prices. The equilibrium price is set to clear the market. The analyst seeks to maximize the expected trading volume by informed investors fol-

FIGURE 1
Timeline



Each informed and uninformed investor is endowed with some initial wealth.

The market for the firm's shares opens and noise traders trade such that they own fraction of $1-\mu_s$ of the firm. Informed and uninformed investors' per capita holdings of the risky asset are D_0^I and D_0^U, respectively. The market clearing price is P_0.

The analyst learns his private signal and issues a forecast, x_R, to the informed investors.

The market for the firm's shares opens and noise traders supply μ_s shares. Informed and uninformed investors' per capita holdings of the risky asset are D_1^I and D_1^U, respectively. The market clearing price is P_1.

The firm value x is realized and investors consume their final wealth.

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[10] One might also think of the analyst's objective function in a broader sense. In particular, his objective function can be interpreted as reflecting the extent to which his forecast changes the recipients' beliefs. Under this interpretation, the model's predictions might speak to a broader set of experts. We thank Paul Fischer for this observation.

lowing the forecast net of costs from forecast errors. When choosing his forecast, the analyst rationally anticipates informed investors' trading strategy and the trading volume it is expected to generate. The trading volume of informed investors equals the magnitude of the change in their holdings at $t = 1$ relative to their holdings at $t = 0$. In order to study the trading volume of the informed investors, we must derive the informed investors' equilibrium holdings at both $t = 1$ and $t = 0$.[11] We begin by solving for the unique linear equilibrium at $t = 1$.

Equilibrium in the Market for the Risky Asset

Three types of investors determine the equilibrium in the market for the risky asset: informed, uninformed, and noise traders. The demand of an uninformed investor at $t = 1$ is a function of the price at $t = 1$. The demand of an informed investor at $t = 1$ is a function of the price at $t = 1$ as well as of the analyst's forecast. We focus on equilibria in which the analyst's forecast fully reveals his private information, Ψ. Hence, in equilibrium, the signal informed investors infer based on the forecast they observe, $\hat{\psi}(x^R)$, equals the analyst's private signal, Ψ. The informed and uninformed investors' demand functions at $t = 1$ are the same as in Grossman and Stiglitz (1980) and are given by:

$$D_1^I(\hat{\psi}(x^R), P_1) = \frac{E(\tilde{x}|\hat{\psi}(x^R)) - P_1}{\rho Var(\tilde{x}|\hat{\psi}(x^R))},$$

$$D_1^U(P_1) = \frac{E(\tilde{x}|P_1) - P_1}{\rho Var(\tilde{x}|P_1)}.$$

Using the market-clearing condition in (2), we can derive the equilibrium price at $t = 1$. The price depends on the analyst's private signal through informed investors' demand function. Since in equilibrium the demand of informed investors is increasing in their inference about the analyst's signal, $\hat{\psi}(x^R)$, the market clearing price is also increasing in $\hat{\psi}(x^R)$. Note that the price does not fully reflect $\hat{\psi}(x^R)$ because the noisy supply garbles the signal. The higher the realization of the noisy supply, the lower is the market clearing price. In the linear equilibrium, the market clearing price can be written as:

$$P_1(\hat{\psi}(x^R), s) = \alpha_0 + \alpha_1 \hat{\psi}(x^R) - \alpha_2 s, \tag{4}$$

where $\alpha_1, \alpha_2 > 0$.

To simplify the notation, whenever our analysis is conducted on the fully revealing equilibrium path, we write both informed investors' demand and the stock price as a function of Ψ rather than a function of $\hat{\psi}(x^R)$.

In a rational expectations equilibrium, strategic investors choose equilibrium holdings at $t = 0$ anticipating the equilibrium, and their equilibrium holdings, at $t = 1$. Since strategic investors have CARA utility, their equilibrium holdings at time $t = 1$ are independent of their wealth and their prior holdings of the risky asset. As a result, at $t = 0$, both informed and uninformed investors choose the same investment strategy even though their investment strategies at $t = 1$ differ. This implies that anticipating private information in the future has no effect on today's trading strategy. In other words, investment decisions are separable in time. Since $D_0^I = D_0^U$

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[11] Grossman and Stiglitz (1980) are not studying the trade generated by strategic investors, but rather focus on the equilibrium price and holdings at $t = 1$. Since the equilibrium holdings of the strategic investors at time $t = 0$ are independent of their prior investment in the risky asset due to their CARA utility function, Grossman and Stiglitz (1980) treat strategic investors' initial holdings as exogenous.

$\equiv D_0$, it follows from the market-clearing condition in (1) that the per-capita holdings of each informed and uninformed investor, D_0, equal μ_s. Lemma 1 formalizes this finding:

Lemma 1: In equilibrium, the per-capita holdings of informed and uninformed investors equal μ_s at $t = 0$.

Informed Investors' Expected Trading Volume

The analyst benefits from trading volume generated by informed investors. However, the analyst cannot perfectly predict informed investors' trading volume because volume varies with the realization of noise traders' supply. For that reason, the analyst considers informed investors' expected trading volume, $\lambda E[|D_1^I(\psi, \tilde{P}_1) - D_0^I| \mid \psi]$, when issuing the forecast. The characteristics of informed investors' expected trading volume will determine the analyst's forecasting strategy. We next derive and provide intuition for how informed investors' expected trading volume varies with the analyst's private information.

For any signal Ψ, informed investors may either buy or sell additional shares—depending on the realization of the noise traders' supply, s. Informed investors buy additional shares if the noise traders' supply is high and sell shares if it is low. Since informed investors' demand is monotonically increasing in the noise traders' supply, s, there exists a realization of noise traders' supply, $s^*(\psi)$, such that informed investors neither buy nor sell shares:

$$s^*(\psi) = \frac{1}{\alpha_2}(\rho Var(\tilde{x}|\psi)\mu_s - E(\tilde{x}|\psi) + \alpha_0 + \alpha_1 \psi). \tag{5}$$

Relying on $s^*(\psi)$, we can write informed investors' expected trading volume, $\Delta(\Psi)$, as the sum of sell and buy transactions:

$$\Delta(\psi) \equiv \lambda E[|D_1^I(\psi,\tilde{P}_1) - D_0^I||\psi] = \lambda \int_{-\infty}^{s^*(\psi)} [D_0^I - D_1^I(\psi,P_1(\psi,s))]\phi_s(s)ds$$

$$+ \lambda \int_{s^*(\psi)}^{\infty} [D_1^I(\psi,P_1(\psi,s)) - D_0^I]\phi_s(s)ds \tag{6}$$

where $\phi_s(s)$ denotes the pdf of the noise traders' supply, $\tilde{s}$. For a given Ψ, the informed investors' trading volume is V-shaped in the noise traders' supply and symmetric around the realization of the noise traders' supply for which no trade occurs, $\tilde{s} = s^*(\psi)$. Integrating the V-shaped function over the noise traders' supply, $\tilde{s}$, yields the expected trading volume informed investors generate for a given Ψ, $\Delta(\Psi)$. The signal Ψ for which this expected trading volume is minimal, must be the one for which the V-shaped function is symmetric around μ_s. We denote this signal by ψ^*. Lemma 2 characterizes the signal, ψ^*, following which informed investors' expected trading volume is minimal:

Lemma 2: Investors' expected trading volume, $\Delta(\Psi)$, is minimal when the analyst's signal equals:

$$\psi^* = \mu_x - \frac{\rho \mu_s}{\tau_\psi}.$$

Following the analyst's signal $\psi = \psi^*$, informed investors' trade is zero when $s = \mu_s$.

Lemma 2 states that informed investors' expected trading volume is minimal when the analyst's signal equals $\psi = \psi^*$. Since $\psi^* < \mu_x$, minimal expected trading volume occurs after obtaining unfavorable news. We next explore the intuition for this result.

Informed investors' expected trading volume is minimal when informed investors' holdings at $t = 1$ remain on average the same as at $t = 0$. When this is the case, uninformed investors' holdings at $t = 1$ also remain on average the same as at $t = 0$ (because noise traders on average trade zero). Moreover, per-capita holdings of informed investors at $t = 1$ will again equal those of uninformed investors because their per-capita holdings were the same at $t = 0$. Hence, we can gain intuition for the result in Lemma 2 by comparing the per-capita holdings of informed and uninformed investors at $t = 1$ for different realizations of the analyst's signal.

First, suppose the analyst's signal is $\psi = \mu_x$. In this case, the informed investors do not revise their beliefs about the firm's earnings prospects. Uninformed investors' beliefs vary with the realization of noise traders' supply, but, on average, also remain unchanged. Nevertheless, the average per-capita holdings of uninformed investors differs from that of informed investors. The reason is that both informed and uninformed investors are risk-averse and informed investors have an informational advantage over uninformed investors. As a result, informed investors are exposed to less residual uncertainty and their per-capita holdings are on average higher than those of uninformed investors when $\psi = \mu_x$.

For values of the analyst's signal other than $\psi = \mu_x$, informed and uninformed investors again hold the same beliefs about the firm's earnings prospects, on average. However, informed investors' expectations about the firm's prospects are more sensitive to a change in Ψ than uninformed investors' expectations. The reason is that uninformed investors can infer only a noisy version of Ψ from the market clearing price. We derived above that informed investors' per-capita holdings are, on average, higher than those of uninformed investors when $\psi = \mu_x$. Hence, the per-capita holdings of informed and uninformed investors are the same for an unfavorable realization of news, $\psi < \mu_x$. In particular, for $\psi = \psi^*$, the news is such that informed and uninformed investors hold the same amount of shares per capita, on average. Therefore, informed investors' expected trading volume is minimal for unfavorable news of $\psi = \psi^*$.

Lemma 3 further characterizes informed investors' expected trading volume. It shows that the expected trading volume of informed investors is a symmetric U-shaped function with its minimum at ψ^*:

Lemma 3: The informed investors' expected trading volume, $\Delta(\Psi)$, has the following properties: (i) strictly positive everywhere, (ii) convex everywhere, (iii) symmetric with respect to $\psi^* = \mu_x - \frac{\rho\mu_s}{\tau_\psi}$, and (iv) the magnitude of the slope converges to the same constant for both Ψ approaching plus and minus infinity.

The characteristics of informed investors' expected trading volume will determine the analyst's forecasting strategy. This is the case even though we consider only equilibria in which informed investors can perfectly infer the analyst's private information. The reason is that the slope of the expected trading volume will affect the analyst's marginal benefit from biasing the forecast. The following section uses the properties of informed investors' expected trading volume, established in Lemma 3, to characterize the analyst's bias function.

Characteristics of the Analyst's Bias Function

When the analyst issues a forecast, he is not confined to tell the truth. However, biasing the forecast is costly to the analyst because it increases his expected costs from forecast errors. As standard in costly signaling models, truth-telling cannot be sustained in equilibrium. The reason is as follows. If the analyst were to tell the truth, informed investors' expected trading volume would vary with the analyst's forecast. Then, the analyst's marginal expected benefit from biasing the forecast would be nonzero, while his marginal expected cost would be zero.

Before we show that there exists an equilibrium in which the analyst's forecast fully reveals his private information, we derive and provide the intuition for several properties of the analyst's forecasting strategy. These properties are necessary for the existence of a fully separating equilibrium. First, we demonstrate that the analyst's forecasting strategy $x^R(\psi)$ has to be continuous in his private information and, hence, the analyst's equilibrium forecast must be strictly increasing in his private signal. Next, we show that not only the analyst's forecast, x^R, but also the forecast bias, $b(\psi)=x^R(\psi)-E[\tilde{x}\,|\,\psi]$, is increasing in the analyst's signal. Nevertheless, the bias is bounded from both below and above. Based on these characteristics, we show that there exists an equilibrium in which the analyst's forecast fully reveals his private information.

Claim 1: Following the signal ψ^*, the analyst will not bias the forecast—i.e., $x^R(\psi^*) = E[\tilde{x}\,|\,\psi^*]$ where $E[\tilde{x}\,|\,\psi^*]=\psi^*=\mu_x-\frac{\rho\mu_s}{\tau_\psi}$.

Since informed investors perfectly infer the analyst's signal, their expected trading volume is minimal following the signal ψ^* (see Lemma 2). Hence, in equilibrium, the analyst is not willing to bear any "signaling costs" in the form of biasing the forecast when he observes $\tilde{\psi}=\psi^*$.

Claim 2: The analyst's forecast $x^R(\psi)$ is continuously increasing in Ψ.

Informed investors' expected trading volume is continuous in Ψ. In equilibrium, the analyst's bias function cannot exhibit a discontinuity. If there were a discontinuity, the expected cost from forecast errors for types just to the right of the discontinuity would differ significantly from the expected cost for types just to the left, while the expected trading volume would be (almost) the same. Therefore, the type with the higher expected cost from forecast errors would always have an incentive to deviate and mimic the other type, which contradicts the existence of an equilibrium with discontinuous bias. Given that the forecast is continuous in the analyst's private information, the analyst's forecast must be strictly monotone in Ψ. This follows from the unbounded support of the analyst's private signal and the unbounded cost from forecast errors.

Claim 3: For any $\psi > \psi^*$ ($\psi < \psi^*$), the analyst's forecast bias $b(\Psi)$ is positive (negative).

Since the analyst's forecast is increasing in Ψ, informed investors infer relatively good news, i.e., $\psi > \psi^*$, from $x^R > x^R(\psi^*)$. When informed investors infer relatively good news ($\psi > \psi^*$) from the analyst's forecast and on average buy shares, the analyst biases the forecast upward to induce informed investors to buy more shares. When informed investors infer relatively bad news ($\psi < \psi^*$) from the analyst's forecast and sell shares, the analyst biases the forecast downward to induce informed investors to sell more shares and increase trading volume.[12]

Claim 4: The analyst's forecast bias $b(\Psi)$ is increasing in Ψ.

From Claims 2 and 3, it follows that the bias is continuously increasing in a neighborhood around ψ^*. Suppose the analyst's bias started to decrease for some signal to the right of ψ^*. If that were the case, then there would be two types that biased their forecasts by the same amount and, hence, incurred the same marginal expected cost from forecast errors. However, the marginal expected benefit from biasing the forecast would be different for the two types. In particular, the marginal expected benefit would be higher for the type for which the bias function is decreasing because informed investors' reaction would be more sensitive to the analyst's forecast. This implies that the analyst's bias cannot be decreasing for signals to the right of ψ^*. Based on a similar argument, we can preclude a decreasing bias for signals to the left of ψ^*.

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[12] If all the strategic investors were informed, i.e., $\lambda = 1$, then the analyst would not bias the forecast. If $\lambda = 0$ informed investors would absorb all the exogenous supply of the noise traders, then the trading volume of informed investors would be independent of their beliefs. In turn, the analyst would minimize his expected costs by issuing an unbiased forecast because he cannot generate additional trading volume by biasing the forecast and altering informed investors' beliefs. We thank an anonymous reviewer for suggesting that we examine the effect of $\lambda = 1$ on the equilibrium.

Claim 5: The analyst's forecast bias $b(\Psi)$ is bounded from below and above.

Part (iv) of Lemma 3 establishes that the marginal expected trading volume converges to a constant. Since the expected trading volume is convex, the slope of the expected trading volume is always smaller in magnitude than its limit. Hence, the marginal benefit from biasing the forecast is bounded, which implies that the marginal expected cost must be bounded as well. The marginal expected cost can only be bounded if the analyst's bias is bounded because the cost function is infinitely steep in its tails.

While the above claims characterize the analyst's forecasting strategy for any equilibrium in which the analyst's forecast fully reveals his private information, they do not establish the existence of such equilibrium. The equilibrium exists if a solution exists to the first-order condition of the analyst's optimization problem in (3). The first-order condition yields:

$$c_A \frac{\partial E[\lambda | D_1^I(\hat{\psi}, P_1(\hat{\psi}, \tilde{s})) - D_0^I || \hat{\psi}]}{\partial \hat{\psi}} \frac{\partial \hat{\psi}(x^R)}{\partial x^R} - E[(x^R - \tilde{x}) | \psi] = 0$$

where $\hat{\psi}(x^R)$ denotes informed investors' inferences about the analyst's private signal based on the forecast he issued. In an equilibrium in which the analyst fully reveals his private information, informed investors' beliefs must be consistent with the analyst's forecasting strategy. That is, $\hat{\psi}(x^R(\psi)) = \psi$. Substituting this equilibrium condition into the first-order condition yields:

$$c_A \frac{\partial E[\lambda | D_1^I(\psi, P_1(\psi, \tilde{s})) - D_0^I || \psi]}{\partial \psi} \frac{1}{\partial x^R(\psi)/\partial \psi} - E[(x^R - \tilde{x}) | \psi] = 0.$$

We can further compute the marginal expected trading volume (see the Appendix) and rewrite the first-order condition as:

$$c_A \lambda (1 - \alpha_1) \tau_\varepsilon [1 - 2\Phi(s^*(\psi))] - \frac{\partial x^R(\psi)}{\partial \psi} E[(x^R - \tilde{x}) | \psi] = 0.$$

This differential equation for $x^R(\psi)$ has a solution with the boundary condition $x^R(\psi^*) = E[\tilde{x} | \psi^*] = \psi^*$. Based on this result and the equilibrium properties of the analyst's forecasting strategy in Claims 1 through 5, we can now establish the existence of an equilibrium in which the analyst's forecast fully reveals his private information.

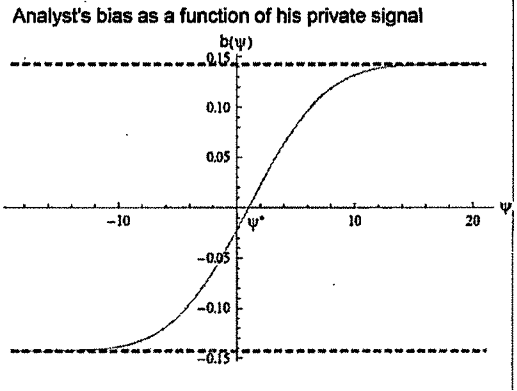
Proposition 1: There exists an equilibrium in which the analyst's forecast fully reveals his private information. In such equilibrium, the analyst's bias function $b(\Psi)$ is increasing, continuous, bounded from above and below, and $b(\psi^*) = 0$.

Informed investors' beliefs are consistent with the analyst's reporting strategy in the sense that informed investors correctly infer the analyst's private information, $\hat{\psi}(x^R(\psi)) = \psi$.

Figure 2 illustrates the analyst's forecasting strategy in the equilibrium of Proposition 1. To generate the figure, we used the following parameter values: $c_A = \rho = \tau_\psi = \tau_\varepsilon = \mu_s = 1$, $\mu_x = 2$, and $\lambda = 0.5$ (which implies $\psi^* = 1$).

So far, we have assumed that the analyst's payoff function is common knowledge and that the information asymmetry between the analyst and informed investors is limited to the analyst's private information, Ψ. In the equilibrium we study, the analyst's forecast fully reveals his private information. In a more realistic setting, one might expect that investors are not able to perfectly

FIGURE 2
The Analyst's Bias as a Function of His Private Signal, Ψ.



infer the analyst's private information because they are uncertain about the analyst's precise objective. The results of this study (both Proposition 1 above and Proposition 2 introduced in Section V) are robust to the introduction of such information asymmetry. More details and formal proofs are available upon request.[13]

IV. EMPIRICAL PREDICTIONS

In practice, analysts are subject to various kinds of incentives. These incentives may arise from trading volume as well as other activities such as investment banking, access to senior management of the companies covered and career concerns. Any specific set of incentives to which an analyst is exposed will affect the properties of the forecast. The empirical predictions we offer focus on the properties of analysts' bias that are due to trading volume alone.

The first prediction of the model is that the analyst biases the forecast upward more often than downward, and that his forecast is on average optimistic.

Corollary 1: The median bias and the expected bias in the analyst's forecast are positive.

The analyst's incentives to bias the forecast arise from the trading volume he expects to generate. These incentives are such that for any private signal $\psi > \psi^*$ the analyst biases the forecast upward, while for any signal $\psi < \psi^*$ the analyst biases the forecast downward. Since the median signal, $\psi = \mu_x$, is greater than $\psi^* = \mu_x - \frac{\rho\mu_s}{\tau_\psi}$, the median bias is upward and the expected bias is positive.

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[13] To verify the robustness of the model to this additional information asymmetry, we introduce an additional random variable to the analyst's payoff function about which investors are uninformed (as in Fischer and Verrecchia [2000] and Dye and Sridhar [2004]). In particular, we assume that the analyst's payoff function is $u^A(x^R, \psi, \upsilon) = c_A \lambda E[|D_1^I(\psi, \tilde{P}_1) - D_0^I| \mid \psi] - E[(x^R - \tilde{x} - \upsilon)^2 \mid \psi]$ where υ is normally distributed with mean zero and precision τ_υ, and is independent of x and Ψ. While the analyst observes the realization of υ before deciding about his forecast, investors do not observe it. The realization of υ may reflect idiosyncratic circumstances that influence the analysts' misreporting costs such as analysts' unknown incentives to "hype up" the firm's stock due to investment banking relations that are unobservable to outside investors. We establish the existence of an equilibrium wherein informed investors are no longer able to perfectly infer the analyst's private signal. Instead, in equilibrium informed investors can infer only the sum of the analyst's posterior expectation and the realization of υ.

The prediction that the forecast is optimistic, on average, is supported by the majority of empirical studies on analysts' earnings forecasts, as surveyed by Kothari (2001). Jackson (2005) documents that analysts trade off accuracy for optimistic bias. The literature also offers other explanations for optimistic analyst forecasts and stock recommendations. For example, Dugar and Nathan (1995), Lin and McNichols (1998), and Michaely and Womack (1999) provide evidence for affiliated analysts being more optimistic than unaffiliated analysts. Alternatively, Lim (2001) suggests that analysts rationally bias their forecast upward in order to obtain better information from the firm's management in the future. Furthermore, Hong and Kubik (2003) show that career concerns can induce overoptimistic forecasts.

In our model, both the analyst's and investors' equilibrium beliefs are fully rational in the sense that they are consistent with Bayes' Rule. Nevertheless, the equilibrium-forecasting behavior resembles the behavior of an analyst who overweights his private information for most realizations of his private signal and truthfully reports his posterior expectations. In the model, the analyst acts as if he underweights his private information only if his private information falls within the interval between $\psi^* = \mu_x - \frac{\rho\mu_s}{\tau_\psi}$ and the prior expectations of the firm's earnings μ_x. Otherwise, the analyst acts as if he overweights his private information.

Corollary 2: For all $\psi \notin [\psi^*, \mu_x]$ the analyst issues a forecast as if he overweights his private information Ψ. That is, for any $\psi \notin [\psi^*, \mu_x]$, there exists a weight $w_\psi > 1$ such that $x^R(\psi) = w_\psi \psi + (1 - w_\psi)\mu_x$. For all $\psi \in [\psi^*, \mu_x]$, the analyst issues a forecast as if he underweights his private information Ψ, i.e., for any $\psi \in [\psi^*, \mu_x]$ there exists a weight $w_\psi \leq 1$ such that $x^R(\psi) = w_\psi \psi + (1 - w_\psi)\mu_x$.

A strand of empirical literature studies the circumstances under which analysts overweight and underweight their private information when forming their forecasts. Chen and Jiang (2006) provide evidence that analysts issue forecasts as if they place larger weight on their private information than we would expect them to do if they followed Bayes' Rule and truthfully reported their posterior expectations (i.e., they act as if they overweight their private information).

Moreover, Chen and Jiang (2006) find that analysts that issue earnings forecasts that are higher than the consensus overweight their private information more than if they issue forecasts that are lower than the consensus. If analysts issue forecasts that are lower than the consensus, they sometimes even underweight their private information. These findings are consistent with the predictions of our model. In particular, our model predicts that the analyst always acts as if he overweights his private information when his private signal exceeds expectations ($\psi > \mu_x$). When his private signal is below expectations ($\psi < \mu_x$), the analyst acts as if he either over- or underweights his private information. For $\psi \in [\psi^*, \mu_x]$, the analyst underweights his private signal (i.e., $w \leq 1$). For $\psi < \psi^*$ the analyst overweights his private signal, as he does for $\psi > \mu_x$. However, the weight the analyst assigns to a negative surprise is lower than the weight he assigns to a positive surprise of the same magnitude (where the surprise is measured as $\psi - \mu_x$).

Chen and Jiang (2006) also find that the deviation from Bayesian weighting increases when the analysts' benefits from doing so are high or when the costs of doing so are low. This empirical finding is also in line with the predictions of our model, in that the analyst is fully rational and his incentives determine the extent to which the analyst acts as if he overweights/underweights his private information.

Friesen and Weller (2006) also study the bias in analysts' earnings forecasts. In particular, they contrast properties of unbiased rational forecasts and forecasts of analysts who are overconfident about the precision of their own information (cognitive bias). Their empirical findings indicate that analysts are overconfident and do not act as if they rationally update their beliefs and truthfully report their expectations. The empirical finding of Friesen and Weller (2006) is consistent with our model's prediction that truth-telling cannot be sustained in equilibrium. Similar to the findings in Chen and Jiang (2006) and Friesen and Weller (2006), Easterwood and Nutt (1999) find that analysts do not act as if they rationally update their beliefs and disclose their expectations without bias. Instead, Easterwood and Nutt (1999) find that analysts over-react to positive information and under-react to negative information. In contrast to Easterwood and Nutt (1999), Bernhardt et al. (2006) find that analysts overweight both positive and negative private information. While our model predicts over-reaction to positive news and to extremely negative news, it predicts under-reaction to slightly negative news ($\psi \in [\psi^*, \mu_x]$). For positive news, our model's prediction is consistent with both Easterwood and Nutt (1999) and Bernhardt et al. (2006). For extremely negative news, our model's prediction is consistent with Bernhardt et al. (2006) but opposite to Easterwood and Nutt (1999). For slightly negative news, our model's prediction is consistent with Easterwood and Nutt (1999) but opposite to Bernhardt et al. (2006). Since both Easterwood and Nutt (1999) and Bernhardt et al. (2006) do not partition their tests according to the magnitude of the negative news, none of their findings necessarily contradicts our model's predictions. We are unaware of any empirical study that differentiates analysts' over-/under-reaction for small and large negative news.

Corollaries 1 and 2 characterized the analyst's forecasting strategy independent of the analyst's characteristics.[14] We next examine how two of the analyst's key characteristics affect the magnitude of the forecast bias and the expected squared forecast error.

In the model, the analyst's incentive to bias the forecast depends on the relative weight, c_A, his objective function assigns to the trading volume the analyst expects to generate. Higher values of c_A provide the analyst with stronger incentives to bias the forecast. As a result, the magnitude of the bias is increasing in c_A not only for each Ψ, but also in expectations. As one would expect, as c_A converges to zero, the analyst's incentive to bias the forecast diminishes and the bias converges to zero for any Ψ. The following Corollary summarizes these predictions.

Corollary 3: In the equilibrium of Proposition 1:

(a) for any $\psi \neq \psi^*$ the absolute value of $b(\tilde{\psi})$ is increasing in c_A;
(b) the expected squared forecast error, $E[FE^2]$, and the expected bias, $E[b(\tilde{\psi})]$, are increasing in c_A; and
(c) for any Ψ, $\lim_{c_A \to 0} b(\psi) = 0$.

In the model, the precision of the analyst's private information, $1 / Var(\tilde{x} \mid \psi) = \tau_\varepsilon$, is given exogenously. In practice, analysts will typically differ with respect to the precision of their private information due to various reasons, e.g., differences in ability, experience, general resources, or access to management.

One might expect that analysts whose private information is of higher precision also exhibit lower squared forecast errors on average. This would unambiguously be the case if analysts refrained from biasing their forecasts. However, because analysts bias their forecast, we need to examine the effect of the precision of the private information on the expected squared forecast error in more detail. The expected squared forecast error can be decomposed into two parts. The first part measures the remaining uncertainty and the second part captures the effect of the bias in the analyst's forecast on the expected squared forecast error:

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[14] If uninformed investors are risk-neutral, then informed investors do not hold any shares in the firm at $t = 0$ and also on average do not hold any shares at $t = 1$. This implies that for $\tilde{\psi} = \mu_x$ informed investors on average do not change their holdings in the firm. As a result, the analyst does not bias the forecast for $\tilde{\psi} = \mu_x$ and his median and expected bias is zero. However, the properties of the analyst's bias function in Claims 1–5 still hold for $\psi^* = \mu_x$. We thank an anonymous reviewer for this observation.

$$E[FE^2] = Var(\tilde{x}|\psi) + E[b(\tilde{\psi})^2]. \tag{7}$$

When the magnitude of the analyst's bias decreases in the precision of his private information, the expected squared forecast error always decreases in the precision of the analyst's private signal. This is the case for sufficiently high precisions of the analyst's information, because informed investors' informational advantage becomes so great that uninformed investors do not trade much in the market. Hence, only informed investors absorb noise traders' supply, and their trading volume hardly varies with the analyst's forecast. As a result, the analyst has fewer incentives to bias the forecast as the precision of his private information increases.

However, when the magnitude of the analyst's bias increases in the precision of his private information, the effect of the precision of the private signal on $E[FE^2]$ is ambiguous. The magnitude of the analyst's bias increases in the precision of his private information whenever the precision is sufficiently small. Intuitively, when the precision is zero, informed investors' trading decision does not depend on the analyst's forecast and, hence, the analyst does not bias the forecast. As the precision increases, informed investors' trading decision depends on the analyst's forecast, providing him with incentives to bias the forecast. As a result, the magnitude of the analyst's bias and the expected squared forecast bias is increasing in the precision of the analyst's private information. In this case, the effect of the precision of the private signal on $E[FE^2]$ depends on the relative magnitude of two opposing effects. If the increase in the bias dominates the decrease in the conditional variance, then the expected squared forecast error increases in the precision of the analyst's private information and *vice versa*. The increase in the bias is higher when the analyst assigns a higher weight to the trading volume—i.e., for higher c_A. Hence, for sufficiently high c_A, an analyst with more precise private information produces larger expected squared forecast errors than an analyst with less precise information, as long as the precision of analysts' private information is relatively low. The following corollary summarizes the effect of the precision of the analyst's private information on the expected squared forecast error:

Corollary 4: In the equilibrium of Proposition 1, there exists c_A^* such that for all $c_A > c_A^*$ the expected squared forecast error increases in τ_ε for sufficiently small values of τ_ε.

To summarize, the model predicts that analysts with more precise private information do not necessarily issue forecasts that result in smaller expected forecast bias and/or smaller expected squared forecast errors. This might shed some light on the surprising empirical findings that affiliated analysts who are conjectured to possess more precise information about a firm do not outperform independent analysts, in the sense of issuing forecasts that result in smaller forecast errors (e.g., Gu and Xue 2008).

V. THE EFFECT OF TRADING COSTS ON THE ANALYST'S FORECAST

The previous section examined properties of the analyst's forecast when the analyst's incentives are tied to the trading volume his forecast generates among informed investors. Analysts' incentives are often tied to the trading volume in the stocks they cover because brokerage houses generate revenues from trading commissions (Cowen et al. 2006). However, in the setting studied in the previous section we assume that investors do not pay any trading commission or, more generally, do not incur any form of trading costs. We made this simplifying assumption because in a Grossman-Stiglitz-like setting in which the analyst provides his forecast to a fraction λ of strategic investors, we can only study trading strategies that are linear in the analyst's forecast. However, fixed per-share trading costs cause informed investors' trading strategy to be nonlinear in the analyst's forecast. To study the effect of per-share trading costs, we resort to a simpler setting. In particular, we assume that only one investor obtains the analyst's forecast. This investor is sufficiently small to act as a price-taker. Apart from the investor who receives the analyst's forecast (the "informed investor"), there is a continuum of uninformed investors. As before, investors have a utility function with constant absolute risk aversion. Given these assumptions, the initial demand of the representative uninformed investor is $D_0^U = \frac{\mu_x - P_0}{\rho}\tau_x$, where $\tau_x = \frac{\tau_\psi \tau_\varepsilon}{\tau_\psi + \tau_\varepsilon}$ and P_0 is the equilibrium stock price. The exogenous per-capita supply of the firm's stock is given by μ_s.[15] In equilibrium, the firm's stock price, P_0, is set such that the per capita demand, D_0^U, equals the per-capita supply μ_s. The equilibrium stock price, that equates demand and supply, is $P_0 = \mu_x - \frac{\rho \mu_s}{\tau_x}$.

In order to capture per-share trading costs, we modify the informed investor's optimization problem. We assume the informed investor incurs per share-trading costs of c_I for each share she buys or sells, i.e., her total trading costs are $c_I|D_1^I - D_0^I|$. After observing the analyst's forecast, x^R, the informed investor updates her beliefs about the firm's earnings in a rational, Bayesian manner and chooses the demand that maximizes her expected utility (8), i.e.:

$$D_1^I(x_R) \in \arg\max_{D_1} E[u^I(D_1^I)|x^R] = -\int_{-\infty}^{\infty} e^{-\rho(W_o + D_1^I(\tilde{x} - P_0) - c_I|D_1^I - D_0^I|)} f(\tilde{x}|x^R)dx. \tag{8}$$

Anticipating her holdings following the analyst's forecast, the investor chooses her optimal initial holdings D_0^I that maximizes her expected utility.

We continue to focus on equilibria where the analyst's forecast fully reveals her private signal. In such equilibria, the informed investor's optimal holding at $t = 1$ depends on the signal that she infers from the analyst's forecast. In the absence of trading costs, the informed investor does not trade for a unique signal, $\psi = \psi^*$, for which her optimal holdings at $t = 1$ remain unchanged relative to her initial holdings D_0^I. For a range of signals close to ψ^* her optimal holdings at $t = 1$ are close to her initial holdings D_0^I, such that her marginal benefit from trading is relatively small.[16] If the informed investor incurs positive marginal trading costs, the trading costs outweigh her benefits from trading for this range of signals. Hence, the informed investor does not trade for this range of signals close to ψ^*. We denote the range of signals for which the informed investor does not trade if she incurs positive marginal trading costs as the "no-trading" zone.

The informed investor chooses her holdings at $t = 0$, anticipating that she will receive the analyst's forecast. In equilibrium, the optimal initial holdings of the informed investor, D_0^I, are the same as the holdings of the representative uninformed investor, D_0^U. As a result, the fact that the informed investor anticipates receiving private information in the future has no effect on her trading strategy today. While we obtain the same result in the previous section, the result that future information receipts do not affect today's trading strategy is even more surprising here. The reason is as follows. In both models, informed investors hold more shares at $t = 1$ than uninformed investors on average, because informed investors have an informational advantage. Since informed investors hold the same amount of shares as uninformed investors initially, this implies that in both models, informed investors buy shares, on average. While trading was assumed to be free in the previous model, this model assumes that the informed investor incurs positive marginal costs from trading. Hence, we might expect the informed investor to optimally hold more shares at $t = 0$ than uninformed investors who do not trade at $t = 1$.

However, this is not the case. The intuition is as follows. The informed investor's initial holdings determine the amount of trading costs she incurs when she wants to adjust her holdings after observing the analyst's forecast. She can minimize her expected costs if she chooses initial holdings equal to her expected future optimal holdings. If she chooses higher initial holdings, then she will save trading costs for good news and incur additional trading costs for bad news. If she chooses lower initial holdings, then she will incur additional trading costs for good news and save trading costs for bad news. Since the informed trader is risk-averse, she chooses initial holdings that guarantee lower trading costs in low-payoff states (resulting in lower variance of final payoff) even though this increases her overall expected trading costs. The informed and uninformed investors have the same risk aversion and, hence, resolve the trade-off between higher expected payoff and lower variance in final payoff in the same way.[17] As a result, the informed trader's initial holdings are the same as those of the representative uninformed investor and the midpoint of the no-trading interval is again $\psi^* = \mu_x - \frac{\rho\mu_s}{\tau_\psi}$.

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[15] Since the informed investor's trade does not affect the market price, her private information is not revealed. For that reason, we do not need noise traders whose trade masks the informed investor's private information.

[16] In these kinds of models, the informed investor's certainty equivalent is quadratic in his holdings such that a small deviation from optimal holdings will also generate small marginal benefits from trading.

As before, the analyst maximizes the informed investor's trading volume net of his expected costs from forecast errors. In contrast to the previous section, the analyst's incentives can more readily be interpreted as maximizing the trading commission his forecast generates for the brokerage house, because the informed investor pays per-share trading commissions.[18] The analyst's objective function is similar to (3) and is given by:

$$u^A(x^R, \psi) = c_A |D_1(x^R) - D_0| - \frac{1}{2}E[(x^R - \tilde{x})|\psi]. \tag{9}$$

In a fully separating equilibrium, the analyst has no incentive to bias the forecast for all signals that fall into the "no-trading" zone because the forecast does not generate any trade. For realizations of his private signal outside the "no-trading" zone, the analyst biases the forecast to induce informed investors to buy more shares for good news and sell more shares for bad news. In fact, his forecasting strategy is qualitatively similar to the forecasting strategy derived as part of Proposition 1. In particular, the analyst's forecast bias is: (1) positive for signals for which the informed investor buys shares and negative for signals for which the informed investor sells shares; (2) weakly increasing in the analyst's private signal; and (3) bounded from above and below.[19]

Proposition 2: There exists an equilibrium in which the analyst's forecast fully reveals his private information. In this equilibrium, the analyst's bias function $b(\Psi)$ is weakly increasing, continuous, bounded from above and below, and zero for $\psi \in [\psi^* - c_I, \psi^* + c_I]$.

In particular, the analyst's equilibrium forecast is given by $x^R(\psi) = E[\tilde{x} \mid \psi] + b(\psi)$, where:[20]

$$b(\psi) = \begin{cases} \frac{c_A\tau_\varepsilon}{\rho}(1+ProductLog(-e^{-(\psi - c_I - \psi^*)\rho/c_A\tau_\varepsilon - 1})) & if\ \psi \geq \psi^* + c_I \\ 0 & if\ \psi \in [\psi^* - c_I, \psi^* + c_I] \\ -\frac{c_A\tau_\varepsilon}{\rho}(1+ProductLog(-e^{-(\psi^* - c_I - \psi)\rho/c_A\tau_\varepsilon - 1})) & if\ \psi < \psi^* - c_I\,. \end{cases}$$

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[17] Uninformed investors face this trade-off with respect to future expected payoffs due to their security holdings.

[18] Depending on the analyst's incentive system, c_A may or may not equal c_I. While we allow for $c_I \neq c_A$, we assume that both c_I and c_A are the same for buys and sells. However, this is only a simplifying assumption and introducing different marginal costs for sells and buys will result in a similar equilibrium.

[19] We omit a formal proof since it follows the same line of arguments as the proofs of Claims 1–5 preceding Proposition 1.

[20] *ProductLog* (x) (also known as Lambert-W or Omega function) is the solution to the differential equation $f'(x) = \frac{f(x)}{x(1+f(x))}$. For a graph of the *ProductLog* function, see the Appendix.

The informed investor's optimal holdings at $t = 1$ are given by:

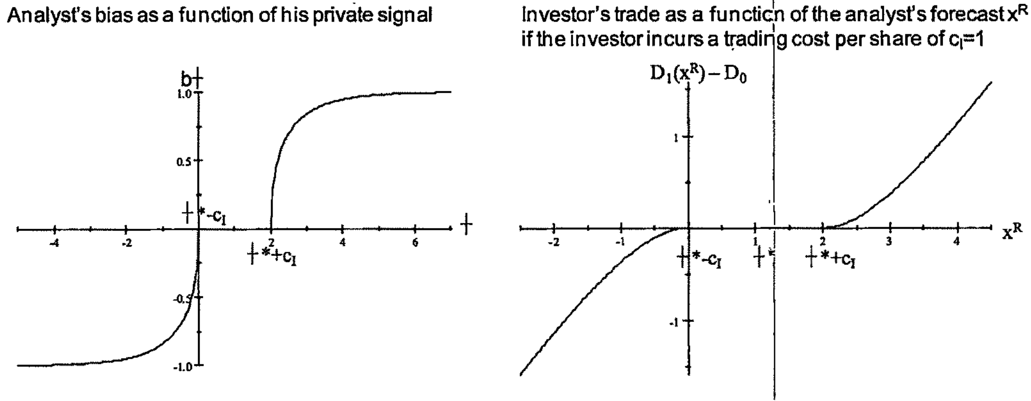
$$D_1(x^R) = \begin{cases} \dfrac{E[\tilde{x}|x^R] - P_0 - c_I}{\rho/\tau_\varepsilon} & if\ x^R > \psi^* + c_I \\ D_0 & if\ \psi^* + c_I > x^R > \psi^* - c_I \\ \dfrac{E[\tilde{x}|x^R] - P_0 + c_I}{\rho/\tau_\varepsilon} & if\ \psi^* - c_I > x^R \end{cases} \tag{10}$$

where $P_0 = \mu_x - \frac{\rho\mu_s}{\tau_x}$. The holdings of the informed investor and the representative uninformed investor at $t = 0$ are $D_0^I = D_0^U = \mu_s$.

Figure 3 illustrates the analyst's bias function and the informed investor's demand when the informed investor faces positive marginal trading costs, c_I. For the plots, we assume the following parameter values: $c_I = c_A = \rho = \tau_\psi = \tau_\varepsilon = \mu_s = 1$; $\mu_x = 2$ (which implies $\psi^* = 1$).

The analyst's forecasting strategy in Proposition 2 is qualitatively similar to the forecasting strategy in Proposition 1. Accordingly, the empirical predictions we derive based in Proposition 1 continue to hold. In particular, even if the informed trader has to pay a per-share trading commission to the analyst, the model continues to predict the following. First, the analyst biases his forecast upward more often than downward and his forecast is on average optimistic. Second, the analyst acts as if he overweights his private information if it is favorable. If the analyst's private information is sufficiently unfavorable, then he also acts as if he overweights his private information, but to a lesser extent. If his private information is slightly unfavorable, then the analyst acts as if he underweights it. Third, the magnitude of the analyst's bias is increasing in the per-share trading commission he receives, c_A. Finally, the model shows that the analyst's expected squared forecast error does not always decrease in the precision of the analyst's information as one might expect at first. Instead, the model continues to predict that the analyst's expected squared forecast error may either increase or decrease in the precision of his private information. In particular, if the precision of his private information is sufficiently low and the per-share trading commission is sufficiently high, a further increase in this precision always increases his expected squared forecast error.

FIGURE 3
Positive Trading Costs


Analyst's bias as a function of his private signal

Investor's trade as a function of the analyst's forecast x^R if the investor incurs a trading cost per share of c_I=1

The only qualitative differences between the forecasting strategies in Propositions 1 and 2 are the signals for which the analyst does not bias the forecast. In the absence of per-share trading costs (Proposition 1), the analyst does not bias the forecast only when his private signal equals ψ^*. In the presence of such trading costs (Proposition 2), the analyst does not bias the forecast for an interval of private signals, which is symmetric around ψ^*. The analyst does not bias the forecast for any signal within this interval because in equilibrium, the analyst's forecast does not generate any trade by the informed investor. Since for these signals the informed investor's trading costs exceed her benefits from trading, the informed investor does not trade—eliminating the analyst's incentive to bias the forecast.

The current section adds to the analysis of the model by providing a concrete example of how analysts' payoff can be tied to the trading volume their forecasts generate. Analysts' incentives are often tied to the trading volume in the stocks they cover because brokerage houses generate revenue from trading commissions. This section analyzes how trading commissions affect investors' trading behavior and, consequently, affect analysts' forecasting strategies. In addition to motivating the assumption that analysts benefit from trading volume, this section demonstrates the robustness of the qualitative characteristics of analysts' forecasting behavior. The robustness of analysts' reporting strategy to significant model modifications, including positive marginal trading costs, suggests that the predictions of the model are likely to capture significant characteristics of analysts' forecasting behavior, whenever analysts' incentives are tied to the trading volume or the revenue from trading commissions analysts' forecasts generate for their brokerage houses.

VI. CONCLUSION

We model the interaction between sell-side analysts and their client-investors. In the model, the analyst issues a forecast to a fraction of strategic investors. The analyst benefits from the trading volume of informed investors that the forecast generates. Since the analyst is not confined to unbiased forecasts, his incentives from trading volume lead the analyst to bias the forecast. In equilibrium, the analyst trades off benefits from trading volume against the cost from forecast errors. We identify and analyze a fully separating equilibrium of this game.

The model provides several empirical predictions, including: (1) the analyst biases the forecast upward more often than downward and the forecast is optimistic, on average; (2) the magnitude of the analyst's bias is increasing in the per-share benefit from trading volume he receives; (3) the analyst's expected squared forecast error does not always decrease (as one might expect), but may also increase in the precision of his private information; and (4) the analyst acts as if he overweights his private information if it is favorable. If his private information is sufficiently unfavorable, then the analyst also acts as if he overweights it, but to a lesser extent. If his private information is slightly unfavorable, then the analyst acts as if he underweights it.

Even though part of the model's predictions are supported by existing empirical studies, the model also provides additional predictions that have not yet been tested empirically. These new predictions set the ground for future empirical work that might shed additional light on the behavior of sell-side analysts and their interaction with client-investors.

APPENDIX

Proof of Lemma 1

Investors' Demand Functions at t = 1

In a fully revealing equilibrium, an informed investor infers the analyst's private information, Ψ, from the analyst's forecast. Therefore, her demand function $D_1^I(\cdot)$ depends on the analyst's signal, Ψ, and the market equilibrium price, P_1. An informed investor's final wealth, $\tilde{W}_2^I = W_1^I + D_1^I(\tilde{x} - P_1)$, conditional on Ψ and P_1, is normally distributed according to:

$$\tilde{W}_2^I|\psi,P_1 \sim N(W_1^I + (E(\tilde{x}|\psi) - P_1)D_1^I,(D_1^I)^2Var(\tilde{x}|\psi)),$$

where W_1^I is the informed investor's wealth at $t = 1$.[21] The informed investor's expected utility at $t = 1$ is given by:

$$E[u^I(D_1^I)|\psi,P_1] = E[e^{-\rho\tilde{W}_2^I}|\psi,P_1] = -e^{-\rho(W_1^I+D_1^I(E(\tilde{x}|\psi)-P_1)-\rho/2(D_1^I)^2Var(\tilde{x}|\psi))}.$$

The demand that maximizes the informed investor's expected utility is:

$$D_1^I(\psi,P_1) = \frac{E(\tilde{x}|\psi) - P_1}{\rho Var(\tilde{x}|\psi)}.$$

Next, we solve for an uninformed investor's demand function. As in Grossman and Stiglitz (1980), we conjecture an equilibrium in which the pricing function at $t = 1$ is linear in both the noise traders' supply, s, and the analyst's private signal, Ψ:

$$P_1(\psi,s) = \alpha_0 + \alpha_1\psi - \alpha_2 s. \tag{11}$$

Since $\tilde{x}$ and $\tilde{P}_1$ are jointly normally distributed, the expected utility of the uninformed trader (given the price P_1) is given by:

$$E[u^U(D_1^U)|P_1] = E[e^{-\rho\tilde{W}_2^U}|P_1] = -e^{-\rho(W_1^U+(E(\tilde{x}|P_1)-P_1)D_1^U-\rho/2(D_1^U)^2Var(\tilde{x}|P_1))}.$$

The demand that maximizes the uninformed investor's expected utility is:

$$D_1^U(P_1) = \frac{E(\tilde{x}|P_1) - P_1}{\rho Var(\tilde{x}|P_1)}.$$

Equilibrium Price at $t = 0$

In order to solve for $P_1(\psi, s)$, we need to compute $E(\tilde{x}\,|\,P_1)$. P_1 is informationally equivalent to $\frac{\lambda E(\tilde{x}|\psi)}{\rho Var(\tilde{x}|\psi)} - s$ or $\psi - \frac{\rho}{\lambda\tau_\varepsilon}s$. Hence:

$$E(\tilde{x}|P_1) = E\left(\tilde{x}|\psi - \frac{\rho}{\lambda\tau_\varepsilon}s\right) = \frac{\psi + \tau_\psi\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2\mu_x - \frac{\rho}{\lambda\tau_\varepsilon}(s - \mu_s)}{1 + \tau_\psi\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2}.$$

Solving the market-clearing condition in (2) for $P_1(\psi, s)$ yields:

$$P_1(\psi,s) = \frac{\frac{\lambda E(\tilde{x}|\psi)}{Var(\tilde{x}|\psi)} + \frac{(1-\lambda)E(\tilde{x}|P_1)}{Var(\tilde{x}|P_1)} - \rho s}{\frac{\lambda}{Var(\tilde{x}|\psi)} + \frac{(1-\lambda)}{Var(\tilde{x}|P_1)}}, \tag{12}$$

where:

$$E(\tilde{x}|\psi) = \psi;$$

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[21] W_1^I is the informed investor's wealth at $t = 1$, i.e., $W_1^I = M_0^I + D_0^I P_1 = M_1^I + D_1^I P_1$ where M_t^I is her investment in the risk-free asset at $t = 0,1$.

$$Var(\tilde{x}|\psi) = \frac{1}{\tau_\varepsilon}; \text{ and}$$

$$Var(\tilde{x}|P_1) = Var\left(\tilde{x}\,\middle|\,\psi - \frac{\rho}{\lambda\tau_\varepsilon}s\right) = \frac{1 + (\tau_\psi + \tau_\varepsilon)\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2}{\tau_\varepsilon\left(1 + \tau_\psi\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2\right)}.$$

Substituting into (12) yields:

$$P_1(\psi,s) = \frac{\left(\lambda(\tau_\psi + \tau_\varepsilon)\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2 + 1\right)\left(\psi - \frac{\rho}{\lambda\tau_\varepsilon}s\right) + (1-\lambda)\left(\tau_\psi\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2\mu_x + \frac{\rho}{\lambda\tau_\varepsilon}\mu_s\right)}{(\tau_\psi + \lambda\tau_\varepsilon)\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2 + 1},$$

and solving for the coefficients of the pricing function $P_1(\psi, s)$ in (11) yields:

$$\alpha_1 = \frac{\lambda(\tau_\psi + \tau_\varepsilon)\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2 + 1}{(\tau_\psi + \lambda\tau_\varepsilon)\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2 + 1};$$

$$\alpha_2 = \alpha_1\frac{\rho}{\lambda\tau_\varepsilon};$$

$$\alpha_0 = \frac{(1-\lambda)\left(\tau_\psi\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2\mu_x + \frac{\rho}{\lambda\tau_\varepsilon}\mu_s\right)}{(\tau_\psi + \lambda\tau_\varepsilon)\left(\frac{\rho}{\lambda\tau_\varepsilon}\right)^2\sigma_s^2 + 1}. \tag{13}$$

Investors' Demand Functions at t = 0

We start by computing the expected utility of both an informed and uninformed investor conditional on P_1. The expected utility of the uninformed investor is:

$$E[u^U(D_1^U)|P_1] = -\, e^{-\rho(W_1^U + D_1^U(E(\tilde{x}|P_1) - P_1) - \rho/2(D_1^U)^2 Var(\tilde{x}|P_1))}\Big|_{D_1^U(P_1) = E(\tilde{x}|P_1) - P_1/\rho Var(\tilde{x}|P_1)} =$$
$$-\, e^{-\rho W_1^U} e^{-(E(\tilde{x}|P_1) - P_1)^2/2Var(\tilde{x}|P_1)},$$

where $W_1^U = W_0^U + D_0^U(P_1 - P_0)$. The expected utility of the informed investor conditional on both P_1 and Ψ is:

$$E[u^I(D_1^I)|\psi, P_1] = -\, e^{-\rho(W_1^I + D_1^I(E(\tilde{x}|\psi) - P_1) - \rho/2(D_1^I)^2 Var(\tilde{x}|\psi))}\Big|_{D_1^I(\psi,P_1) = E(\tilde{x}|\psi) - P_1/\rho Var(\tilde{x}|\psi)} =$$
$$-\, e^{-\rho W_1^I} e^{-(E(\tilde{x}|\psi) - P_1)^2/2Var(\tilde{x}|\psi)},$$

where $W_1^I = W_0^I + D_0^I(P_1 - P_0)$. Since $\tilde{\psi}\,|\,P_1$ is normally distributed, we can define:

$$\tilde{Z} = \left.\frac{E(\tilde{x}|\psi) - P_1}{\sqrt{Var(E(\tilde{x}|\psi)|P_1)}}\right|_{P_1},$$

where $\tilde{Z}$ is distributed normally with mean $\mu_z = \frac{E(\tilde{x}|P_1)-P_1}{\sqrt{Var(E(\tilde{x}|\psi)|P_1)}}$ and variance 1. $\tilde{Z}^2$ is distributed non-central Chi-square with one degree of freedom and parameter $k = \mu_z^2$. The moment-generating function for the non-central Chi-square distribution is $E[e^{-t\tilde{Z}^2}] = \frac{1}{\sqrt{1+2t}} \exp\{-\frac{t}{1+2t}\mu_z^2\}$, where $t = \frac{Var(E(\tilde{x}|\psi)|P_1)}{2Var(\tilde{x}|\psi)}$. Simplification yields;

$$E[e^{-t\tilde{Z}^2}] = \frac{1}{\sqrt{1+2\frac{Var(E(\tilde{x}|\psi)|P_1)}{2Var(\tilde{x}|\psi)}}} \exp\left\{-\frac{\frac{Var(E(\tilde{x}|\psi)|P_1)}{2Var(\tilde{x}|\psi)}}{1+2\frac{Var(E(\tilde{x}|\psi)|P_1)}{2Var(\tilde{x}|\psi)}} \frac{(E(\tilde{x}|P_1) - P_1)^2}{Var(E(\tilde{x}|\psi)|P_1)}\right\}$$

$$= \sqrt{\frac{Var(\tilde{x}|\psi)}{Var(\tilde{x}|P_1)}} \exp\left\{-\frac{(E(\tilde{x}|P_1) - P_1)^2}{2Var(\tilde{x}|P_1)}\right\},$$

where the last equality follows from $Var(\tilde{x} \mid \psi) + Var(E(\tilde{x} \mid \psi) \mid P_1) = Var(\tilde{x} \mid P_1)$. With this we can compute the expected utility of an informed trader conditional on P_1:

$$E[u^I(D_1^I)|P_1] = -e^{-\rho W_1^I} E[e^{-(E(\tilde{x}|\psi) - P_1)^2/2Var(\tilde{x}|\psi)}|P_1] =$$

$$-e^{-\rho W_1^I} \sqrt{\frac{Var(\tilde{x}|\psi)}{Var(\tilde{x}|P_1)}} e^{-(E(\tilde{x}|P_1) - P_1)^2/2Var(\tilde{x}|P_1)}.$$

Next, we derive the first-order condition for the informed and uninformed investors' holdings at $t = 0$. The first derivative of the uninformed investor's expected utility with respect to D_0^U is:

$$\frac{\partial E[u^U(D_1^U)]}{\partial D_0^U} = \frac{\partial}{\partial D_0^U} \int_{P_1} E[u^U(D_1^U)|P_1] \phi_{P_1}(P_1) dP_1 = \int_{P_1} \frac{\partial}{\partial D_0^U} E[u^U(D_1^U)|P_1] \phi_{P_1}(P_1) dP_1,$$

where $\phi_{P_1}(\cdot)$ is the pdf of $\tilde{P}_1$. Hence, the FOC for the uninformed investor's holding at $t = 0$ is:

$$e^{-\rho W_0^U} \int_{P_1} \rho(P_1 - P_0) e^{-\rho D_0^U (P_1 - P_0)} e^{-(E(\tilde{x}|P_1) - P_1)^2/2Var(\tilde{x}|P_1)} \phi_{P_1}(P_1) dP_1 = 0.$$

The first derivative of the informed investor's expected utility with respect to D_0^I is:

$$\frac{\partial E[u^I(D_1^I)]}{\partial D_0^I} = \frac{\partial}{\partial D_0^I} \int_{P_1} E[u^I(D_1^I)|P_1] \phi_{P_1}(P_1) dP_1 = \int_{P_1} \frac{\partial}{\partial D_0^I} E[u^I(D_1^I)|P_1] \phi_{P_1}(P_1) dP_1$$

and, hence, the FOC for the informed investor's holding at $t = 0$ is:

$$e^{-\rho W_0^I} \sqrt{\frac{Var(\tilde{x}|\psi)}{Var(\tilde{x}|P_1)}} \int_{P_1} \rho(P_1 - P_0) e^{-\rho D_0^I (P_1 - P_0)} e^{-(E(\tilde{x}|P_1) - P_1)^2/2Var(\tilde{x}|P_1)} \phi_{P_1}(P_1) dP_1 = 0.$$

In addition, for $D_0^i \to -\infty(\infty)$, $i=I,U$, an investor's marginal expected utility with respect to D_0^i converges to ∞ $(-\infty)$ and her expected utility converges to $-\infty$. This, together with the fact that the second derivative of the expected utility is negative for all D_0^i, implies that there exists a unique optimal $D_0^i(P_0)$ for both informed and uninformed investors. Since both FOCs are identical up to a constant multiplier, the optimal demand function $D_0(P_0)$ is the same for both informed and uninformed investors. In particular, the market-clearing condition in (1) yields:

$$D_0 = \mu_s.$$

Proof of Lemma 2

The proof of Lemma 2 follows along the same lines as the proof of Lemma 3, part (iii) and is omitted for brevity.

Proof of Lemma 3

Part (i)

s^* (ψ) is given in Equation (5). For $s = s^*$ (ψ), no trade occurs—i.e., $D_1^I(\psi, P_1(\psi, s^*(\psi))) = D_0^I$. Both integrands in (6) are zero for $s = s^*$ (ψ) and strictly positive elsewhere for any Ψ. Hence, the expected trading volume is strictly positive for any Ψ.

Part (ii)

The first derivative of the expected trading volume with respect to Ψ is:

$$\begin{aligned}\frac{\partial E[\lambda|D_1^I(\psi,P_1) - D_0^I||\psi]}{\partial\psi} &= -\lambda\int_{-\infty}^{s^*(\psi)} \frac{\partial}{\partial\psi}D_1^I(\psi,P_1)\phi_s(s)ds + \lambda\frac{\partial s^*(\psi)}{\partial\psi}[D_0^I \\ &\quad - D_1^I(\psi,P_1(\psi,s^*(\psi)))]\phi_s(s^*(\psi)) + \lambda\int_{s^*(\psi)}^{\infty} \frac{\partial}{\partial\psi}D_1^I(\psi,P_1)\phi_s(s)ds \\ &\quad - \lambda\frac{\partial s^*(\psi)}{\partial\psi}[D_1^I(\psi,P_1(\psi,s^*(\psi))) - D_0^I]\phi_s(s^*(\psi)) \\ &= -\lambda\int_{-\infty}^{s^*(\psi)} (1-\alpha_1)\tau_\varepsilon\phi_s(s)ds + 0 + \lambda\int_{s^*(\psi)}^{\infty} (1-\alpha_1)\tau_\varepsilon\phi_s(s)ds - 0 \\ &= \lambda(1-\alpha_1)\tau_\varepsilon[1 - 2\Phi_s(s^*(\psi))].\end{aligned}$$

Straightforward algebra shows that $\alpha_1 < 1$. This together with $E[\tilde{x}|\ \psi] = \psi$ implies that s^* (ψ) is decreasing in Ψ. Hence, the first derivative is negative for Ψ lower than some threshold and positive for Ψ higher than the threshold. The second derivative of the expected trading volume with respect to Ψ yields:

$$\frac{\partial^2 E[\lambda|D_1^I(\psi,P_1) - D_0^I||\psi]}{\partial\psi^2} = -2\lambda(1-\alpha_1)\tau_\varepsilon\phi_s(s^*(\psi))\frac{\partial s^*(\psi)}{\partial\psi} > 0$$

which follows again from s^* (ψ) being decreasing in Ψ.

Part (iii)

We prove the symmetry by showing that the derivative of the expected trading volume with respect to Ψ is of the same magnitude but of opposite sign for $\psi^* + v$ and $\psi^* - v$ for all $v > 0$. First, we show that $s^*(\psi^*) = \mu_s$ where $\psi^* = \mu_x - \frac{\rho\mu_s}{\tau_\psi}$:

$$\begin{aligned}s^*(\psi^*) &= \frac{1}{\alpha_2}(\rho Var(\tilde{x}|\psi^*)D_0 - E(\tilde{x}|\psi^*) + \alpha_0 + \alpha_1\psi^*) \\ &= \frac{1}{\alpha_2}\left(\frac{\rho}{\tau_\varepsilon}\mu_s - (1-\alpha_1)\left(\mu_x - \frac{\rho\mu_s}{\tau_\psi}\right) + \alpha_0\right) = \mu_s\end{aligned}$$

where the last equality follows from substituting the expressions for α_0 and α_1 from (13). Then,

$s^*(\psi^* \pm v) = \mu_s \pm (\alpha_1 - 1)v$. For $\psi = \psi^* + v$ the first derivative of the expected trading volume yields:

$$\frac{\partial \lambda E[|D_1^I(\psi, \tilde{P}_1) - D_0^I||\psi = \psi^* + v]}{\partial \psi} = \lambda(1 - \alpha_1)\tau_\varepsilon[1 - 2\Phi_s(\mu_s + (\alpha_1 - 1)v)].$$

For $\psi = \psi^* - v$, the first derivative of the expected trading volume similarly yields:

$$\begin{aligned}\frac{\partial \lambda E[|D_1^I(\psi, \tilde{P}_1) - D_0^I||\psi = \psi^* - v]}{\partial \psi} &= \lambda(1 - \alpha_1)\tau_\varepsilon[1 - 2\Phi_s(\mu_s - (\alpha_1 - 1)v)] \\ &= -\lambda(1 - \alpha_1)\tau_\varepsilon[1 - 2\Phi_s(\mu_s + (\alpha_1 - 1)v)].\end{aligned}$$

Hence, the derivative of the expected trading volume with respect to Ψ is of the same magnitude but of opposite sign for $\psi^* + v$ and $\psi^* - v$. This proves that the expected trading volume is symmetric around ψ^*. As a result, the expected trading volume has its unique local minimum at ψ^*.

Part (iv)

The limit of the slope of the expected trading volume as $\Psi \to \infty$ is:

$$\lim_{\psi \to \infty} \frac{\partial \lambda E[|D_1^I(\psi, \tilde{P}_1) - D_0^I||\psi]}{\partial \psi} = \lambda(1 - \alpha_1)\tau_\varepsilon.$$

From symmetry, it follows that:

$$\lim_{\psi \to -\infty} \frac{\partial \lambda E[|D_1^I(\psi, \tilde{P}_1) - D_0^I||\psi]}{\partial \psi} = -\lambda(1 - \alpha_1)\tau_\varepsilon.$$

Proof of Proposition 1—Analyst's Forecasting Strategy

$$u^A(x^R, \psi) = \lambda c_A E[|D_1^I(x^R, P_1(x^R, \tilde{s})) - D_0^I||x^R] - \frac{1}{2}E[(x^R - \tilde{x})^2|\psi]$$

Let $\hat{\psi}(x^R)$ denote the signal Ψ that informed investors infer from a report x^R:

$$\begin{aligned}E[|D_1^I(\hat{\psi}(x^R), P_1(\hat{\psi}(x^R), \tilde{s})) - D_0^I||x^R] = &\int_{-\infty}^{s^*(\hat{\psi}(x^R))} (D_0^I - D_1^I(\hat{\psi}(x^R), P_1(\hat{\psi}(x^R), \tilde{s})))\phi(s)ds \\ &+ \int_{s^*(\hat{\psi}(x^R))}^{\infty} (D_1^I(\hat{\psi}(x^R), P_1(\hat{\psi}(x^R), \tilde{s})) - D_0^I)\phi(s)ds\end{aligned}$$

The first-order condition yields:

$$\frac{\partial \lambda c_A E[|D_1^I(\hat{\psi}(x^R), P_1(\hat{\psi}(x^R), \tilde{s})) - D_0^I||x^R]}{\partial x^R} - E[(x^R - \tilde{x})|\psi] = 0$$

$$\frac{\partial \lambda c_A E[|D_1^I(\hat{\psi}(x^R), P_1(\hat{\psi}(x^R), \tilde{s})) - D_0^I||x^R]}{\partial \hat{\psi}} \frac{\partial \hat{\psi}(x^R)}{\partial x^R} - E[(x^R - \tilde{x})|\psi] = 0$$

$$\lambda c_A(1-\alpha_1)\tau_\varepsilon[1-2\Phi_s(s^*(\hat{\psi}(x^R)))]\frac{\partial\hat{\psi}(x^R)}{\partial x^R}-E[(x^R-\bar{x})|\psi]=0.$$

For notational convenience, let $g(\psi)\equiv\lambda c_A(1-\alpha_1)\tau_\varepsilon[1-2\Phi_s(s^*(\psi))]$. In equilibrium, we have $\hat{\psi}(x^R(\psi))=\psi$ and, hence, $\hat{\psi}'(x^R)=1/\partial x^R(\psi)/\partial\psi$. Moreover, $\partial x^R(\psi)/\partial\psi=b'(\psi)+1$ and $E[(x^R-\bar{x})|\psi]=b(\psi)$. So, the first-order condition yields:

$$g(\psi)-(b'(\psi)+1)b(\psi)=0 \text{ with } b(\psi^*)=0. \tag{14}$$

The first-order condition provides an ordinary differential equation (ODE) for the analyst's bias function $b(\Psi)$. Claim 1 provides the boundary condition $b(\psi^*)=0$. Next, we substitute $c(\psi)=1/2(b(\psi))^2$. We begin by considering the case of $\psi\geq\psi^*$. From Claim 3, we know that $b\geq 0$. Then, $b(\psi)=\sqrt{2|c(\psi)|}$ and the first-order condition in (14) can be rewritten as:

$$c'(\psi)=g(\psi)-\sqrt{2|c(\psi)|} \text{ with } c(\psi^*)=0. \tag{15}$$

Since the ODE is not Lipschitz continuous at ψ^*, we appeal to the theorem by Cauchy-Peano for existence of a solution.[22] The Cauchy-Peano Theorem requires that the right-hand side of the ODE is continuous. This is the case for the ODE in (15) and, hence, there exists a function $c(\Psi)$ satisfying the initial value problem in (15).

Next, we show that $c(\Psi)$ provides a solution to the analyst's forecasting problem. That requires $c(\Psi)\geq 0$ such that $b(\Psi)$ is real. Note that if $c(\Psi)=0$ for some $\psi>\psi^*$, then $c'(\psi)>0$ because $g(\Psi)>0$ for all $\psi>\psi^*$ (recall that $s^*(\psi^*)=\mu_s$ and $s^*(\psi)$ is decreasing in Ψ). This means that if $c(\psi_1)=0$ for some $\psi_1>\psi^*$, then $c(\Psi)\geq 0$ for all $\psi>\psi_1$. Since $c(\psi^*)=0$, we know $c(\Psi)\geq 0$ for all $\psi\geq\psi^*$. We next want to show that $c(\Psi)>0$ for $\psi>\psi^*$. Suppose that $c(\psi_1)=0$ for $\psi_1>\psi^*$. From $g(\psi_1)>0$, it follows that $c'(\psi_1)>0$, which implies that $c(\Psi)<0$ for some $\psi<\psi_1$, a contradiction. The solution to the initial value problem in (15) is therefore strictly positive for $\psi>\psi^*$. A similar argument (using $g(\psi)<0$ for $\psi<\psi^*$) shows that the solution c is positive for $\psi<\psi^*$ as well.

Since $c(\Psi)\geq 0$, we may now write $b(\Psi)$ as $b(\psi)=\sqrt{2c(\psi)}$ for $\psi>\psi^*$ and $b(\psi)=-\sqrt{2c(\psi)}$ for $\psi<\psi^*$ (see Claim 3). As a final step, we show that the derivative of $c(\Psi)$ is continuous. We again start by focusing on $\psi\geq\psi^*$. Since $b'(\psi)=\frac{c'(\psi)}{\sqrt{2c(\psi)}}$ and $c(\psi)>0$ for $\psi>\psi^*$, $b'(\psi)$ is continuous for $\psi>\psi^*$. Suppose that $b(\Psi)$ is not bounded as $\psi\rightarrow\psi^*$. This means that there is a sequence $\psi_k\downarrow\psi^*$ such that $b'(\psi_k)\rightarrow\infty$, and so:

$$\frac{b(\psi_k)}{\psi_k-\psi^*}\rightarrow\infty \text{ as } k\rightarrow\infty.$$

Dividing Equation (14) by $(\psi_k-\psi^*)$ and rearranging yields:

$$\frac{g(\psi_k)}{\psi_k-\psi^*}=(b'(\psi_k)+1)\frac{b(\psi_k)}{\psi_k-\psi^*}.$$

Taking limits on both sides yields $g'(\psi^*)=\infty$, which is a contradiction because we know that:

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[22] Cauchy-Peano Existence Theorem: Suppose that F is a function defined on $I_t\times U_p$, where I is an open interval in R and U is an open set in R^n. Suppose now that F is continuously differentiable in t and continuous (not necessarily Lipschitz) in p. Then given any $t_0\in I$ and $p\in U$, there exists an $\varepsilon>0$ such that the initial value problem, $x'(t)=F(t,x(t))$, $x(t_0)=p$, has a solution defined on the interval $(t_0-\varepsilon, t_0+\varepsilon)$. For a proof of the theorem, see Coddington and Levinson (1955, 6).

$$g'(\psi^*) = 2\lambda(1-\alpha_1)\tau_\varepsilon\phi(0)\frac{1}{\sigma_s}\frac{1-\alpha_1}{\alpha_2}.$$

From this it follows that there exists a constant C such that $b'(\psi)$ is bounded by C and $b(\Psi)$ is bounded by $C\ (\psi - \psi^*)$ as $\psi_k \downarrow \psi^*$. We next show that $b'(\psi)$ has a limit. Taking limits, we can see that $g'(\psi^*) = \beta(1+\beta)$, where $\beta \equiv \lim_{\psi_k \downarrow \psi^*}\frac{b(\psi_k)}{\psi_k - \psi^*}$. This equation has two solutions, only one of which is positive (we know from Claim 4 that $b'(\psi) \geq 0$). To conclude that $b'(\psi)$ is continuous at ψ^*, we now observe that for any sequence $\psi_k \downarrow \psi^*$, we may extract a convergent subsequence and this subsequence must converge to the solution for β. Thus $b'(\psi) \rightarrow \beta$ as $\psi_k \downarrow \psi^*$ and so $b'(\psi)$ is continuous from the right at ψ^*.

An identical argument shows that b is defined for $\psi \leq \psi^*$ and continuous from the left at ψ^*. The value for $b'(\psi^*)$ will again be the positive root of the quadratic equation $g'(\psi^*) = \beta(1 + \beta)$, such that $b'(\psi)$ will also be continuous. This completes the proof.

Empirical Predictions

Proof of Corollary 1

Since the expected trade function is symmetric with respect to ψ^* and the expected forecast costs are symmetric around 0, the bias function $b(\Psi)$ is symmetric around ψ^* and $b\ (\psi) = -b\ (2\psi^* - \psi)$. Given that $\psi^* < \mu_x$ and the symmetry of the distribution function of Ψ around μ_x the pdf of Ψ is higher than the pdf of $2\psi^* - \psi$ for any $\psi > \psi^*$. Hence, the expected bias is positive.

Proof of Corollary 2

A sufficient condition for the corollary to hold is that the analyst's bias is positive for $\psi > \psi^*$ and negative for $\psi < \psi^*$. Proposition 1 states that this condition holds in equilibrium, which completes the proof of the corollary.

Proof of Corollary 3

Part (a). Since the marginal benefit from increasing the forecast is linear in c_A and the marginal cost is independent of c_A, the magnitude of the analyst's bias, $|b(\psi)|$, is increasing in c_A for all $\psi \neq \psi^*$.

Part (b). We first want to show that $\frac{\partial E[-FE]}{\partial c_A} = \frac{\partial E[b(\psi)]}{\partial c_A} > 0$. We have:

$$E[b(\tilde{\psi})] = \int_{\psi^*}^{\infty} b(\psi;c_A)[f(\psi) - f(2\psi^* - \psi)]d\psi$$

and, hence:

$$\frac{\partial E[b(\tilde{\psi})]}{\partial c_A} = \int_{\psi^*}^{\infty} \frac{\partial b(\psi;c_A)}{\partial c_A}[f(\psi) - f(2\psi^* - \psi)]d\psi > 0$$

because $\frac{\partial b(\psi;c_A)}{\partial c_A} > 0$ and both $[f\ (\psi) - f\ (2\psi^* - \psi)]$ and ψ^* are independent of c_A.

Next, we show that $\partial E[FE^2]/\partial c_A > 0$. Since the magnitude of $b(\Psi)$ is increasing in c_A for all $\psi \neq \psi^*$, the expected squared bias is also increasing in c_A. The conditional variance of x does not depend on c_A.

Part (c). Since the marginal benefit from increasing the forecast is finite, the analyst's marginal benefit approaches 0 as $c_A \rightarrow 0$. It follows that, in equilibrium, the analyst's expected marginal cost from biasing the forecast also approaches zero as $c_A \rightarrow 0$ and, hence, $\lim_{c_A \rightarrow 0} b(\psi) = 0$.

Proof of Corollary 4

As claimed before, as the precision of the private information, τ_ε, goes to zero, the expected bias of the analyst goes to zero as well (since the trade generated by the analyst's forecast goes to zero). This holds for any finite weight that the analyst's utility assigns to the trading volume, c_A. Formally, $\lim_{\tau_\varepsilon \to 0} b(\psi) = 0$ for any $c_A \in [0, \infty)$ and any Ψ. Also, $\lim_{c_A \to \infty} E[b(\tilde{\psi})] = \infty$ for any $\tau_\varepsilon > 0$ and, hence, $\lim_{c_A \to \infty} E[b(\tilde{\psi})^2] = \infty$. Moreover, for any $\psi \neq \psi^*$ we know from Corollary 3 that $b(\psi)^2$ is increasing in c_A. From continuity it follows that there exists a sufficiently high c_A^*, such that for all $c_A > c_A^*$ the expected squared bias increases in τ_ε, for sufficiently small values of τ_ε, faster than the decrease in $Var(\tilde{x} \mid \psi) = 1/\tau_\varepsilon$. This, together with Equation (7) implies that there exists c_A^* such that for all $c_A > c_A^*$ the expected squared forecast error increases in τ_ε for sufficiently small values of τ_ε.

Proof of Proposition 2

Informed Investor's Demand Function at t = 0

We can compute $D_1^I(\psi)$ as:

$$D_1^I(\psi) = \begin{cases} \dfrac{E[\tilde{x}|x^R] - P_0 - c_I}{\rho/\tau_\varepsilon} & \text{if } \psi > \psi^* + c_I \\ D_0^I & \text{if } \psi^* + c_I > \psi > \psi^* - c_I \\ \dfrac{E[\tilde{x}|x^R] - P_0 + c_I}{\rho/\tau_\varepsilon} & \text{if } \psi^* - c_I > \psi, \end{cases}$$

where:

$$\psi^* = \frac{D_0 \rho}{\tau_\varepsilon} + P_0. \tag{16}$$

For notational convenience, let the no-trading zone be denoted by $\Psi_{NT} = [\underline{\psi}, \bar{\psi}]$, where $\underline{\psi} = \psi^* - c_I$ and $\bar{\psi} = \psi^* + c_I$. Given the optimal $D_1^I(\psi)$, we can compute the informed investor's *ex ante* expected utility as:

$$-\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} e^{-\rho[W_0 + (\tilde{x} - P_0)D_1^I - c_I|D_1^I - D_0^I|]} f(x|\psi)h(\psi)dxd\psi =$$

$$-\int_{-\infty}^{\underline{\psi}} e^{-\rho\left[W_0 + (E[\tilde{x}|\psi] - P_0)\frac{E[\tilde{x}|\psi] - P_0 + c_I}{\rho Var(\tilde{x}|\psi)} + c_I\left(\frac{E[\tilde{x}|\psi] - P_0 + c_I}{\rho Var(\tilde{x}|\psi)} - D_0^I\right) - \frac{\rho}{2}\left(\frac{E[\tilde{x}|\psi] - P_0 + c_I}{\rho Var(\tilde{x}|\psi)}\right)^2 Var(\tilde{x}|\psi)\right]} h(\psi)d\psi$$

$$-\int_{\underline{\psi}}^{\bar{\psi}} e^{-\rho\left[W_0 + (E[\tilde{x}|\psi] - P_0)D_0^I - \frac{\rho}{2}D_0^2 Var(\tilde{x}|\psi)\right]} h(\psi)d\psi$$

$$-\int_{\bar{\psi}}^{\infty} e^{-\rho\left[W_0 + (E[\tilde{x}|\psi] - P_0)\frac{E[\tilde{x}|\psi] - P_0 + c_I}{\rho Var(\tilde{x}|\psi)} - c_I\left(\frac{E[\tilde{x}|\psi] - P_0 + c_I}{\rho Var(\tilde{x}|\psi)} - D_0^I\right) - \frac{\rho}{2}\left(\frac{E[\tilde{x}|\psi] - P_0 + c_I}{\rho Var(\tilde{x}|\psi)}\right)^2 Var(\tilde{x}|\psi)\right]} h(\psi)d\psi, \tag{17}$$

where $f(x|\psi)$ is the conditional distribution of x and $h(\Psi)$ is the prior distribution of Ψ. Let:

$$MV(\psi)|_{c_I = 0} = W_0 + \frac{1}{2}\frac{(E[\tilde{x}|\psi] - P_0)^2}{\rho Var(\tilde{x}|\psi)}$$

denote the mean-variance term if $c_I = 0$. WLOG, we set $W_0 = 0$ for the remainder of the proof. We can then rewrite (17) as:

$$-\int_{-\infty}^{\underline{\psi}} e^{-\rho[MV(\psi)|_{c_I=0}-c_I(D_0^I-E[\tilde{x}|\psi]-P_0+1/2c_I/\rho Var(\tilde{x}|\psi))]}h(\psi)d\psi$$

$$-\int_{\underline{\psi}}^{\bar{\psi}} e^{-\rho[MV(\psi)|_{c_I=0}+(E[\tilde{x}|\psi]-P_0-\rho/2D_0^I Var(\tilde{x}|\psi))D_0^I-1/2(E[\tilde{x}|\psi]-P_0)^2/\rho Var(\tilde{x}|\psi)]}h(\psi)d\psi$$

$$-\int_{\bar{\psi}}^{\infty} e^{-\rho[MV(\psi)|_{c_I=0}-c_I(E[\tilde{x}|\psi]-P_0-1/2c_I\rho Var(\tilde{x}|\psi)-D_0^I)]}h(\psi)d\psi. \tag{18}$$

The informed investor chooses D_0^I to maximize her *ex ante* expected utility, taking into account her optimal trading strategy at $t = 1$. Equation (16) shows a one-to-one relation between ψ^* and D_0^I. In particular, $D_0^I = E[\tilde{x} \mid \psi^*] - P_0 / \rho Var(\tilde{x} \mid \psi)$. Hence, solving for the optimal D_0 is equivalent to solving for the optimal ψ^*. Rewriting (18) yields:

$$-\int_{-\infty}^{\underline{\psi}} e^{-\rho[MV(\psi)|_{c_I=0}-c_I(E[\tilde{x}|\psi^*]-E[\tilde{x}|\psi]-1/2c_I/\rho Var(\tilde{x}|\psi))]}h(\psi)d\psi$$

$$-\int_{\underline{\psi}}^{\bar{\psi}} e^{-\rho[MV(\psi)|_{c_I=0}-1/2(E[\tilde{x}|\psi^*]-E[\tilde{x}|\psi])^2/\rho Var(\tilde{x}|\psi)]}h(\psi)d\psi$$

$$-\int_{\bar{\psi}}^{\infty} e^{-\rho[MV(\psi)|_{c_I=0}-c_I(E[\tilde{x}|\psi]-E[\tilde{x}|\psi^*]-1/2c_I/\rho Var(\tilde{x}\psi))]}h(\psi)d\psi.$$

Further:

$$\begin{aligned}
e^{-\rho MV(\psi)|_{c_I=0}}h(\psi) &\propto \exp\left\{-\frac{1}{2}\frac{(E[\tilde{x}|\psi]-P_0)^2}{Var(\tilde{x}|\psi)}\right\}\exp\left\{-\frac{\tau_\psi}{2}(\psi-\mu_x)^2\right\} \\
&= \exp\left\{-\frac{1}{2}(\tau_\varepsilon(\psi-P_0)^2+\tau_\psi(\psi-\mu_x)^2)\right\} \\
&= \exp\left\{-\frac{1}{2}((\tau_\varepsilon+\tau_\psi)\psi^2+\tau_\psi\mu_x^2+\tau_\varepsilon P_0^2-2P_0\psi\tau_\varepsilon-2\mu_x\psi\tau_\psi)\right\} \\
&= \exp\left\{-\frac{1}{2}\left((\tau_\varepsilon+\tau_\psi)\left(\psi-\frac{P_0\tau_\varepsilon+\mu_x\tau_\psi}{\tau_\varepsilon+\tau_\psi}\right)^2+\frac{\tau_\varepsilon\tau_\psi}{\tau_\varepsilon+\tau_\psi}(\mu_x-P_0)^2\right)\right\} \\
&= \exp\left\{-\frac{1}{2}\left((\tau_\varepsilon+\tau_\psi)\left(\psi-\frac{P_0\tau_\varepsilon+\mu_x\tau_\psi}{\tau_\varepsilon+\tau_\psi}\right)^2\right)\right\}\exp\left\{-\frac{1}{2}\frac{\tau_\varepsilon\tau_\psi}{\tau_\varepsilon+\tau_\psi}(\mu_x-P_0)^2\right\} \\
&\propto \phi\left(\sqrt{\tau_\varepsilon+\tau_\psi}\left(\psi-\frac{P_0\tau_\varepsilon+\mu_x\tau_\psi}{\tau_\varepsilon+\tau_\psi}\right)\right),
\end{aligned}$$

where $\phi()$ denotes the pdf of a standard normal distribution. Hence, the informed investor's objective is to choose ψ^* that maximizes:

$$\max_{\psi^*}\int_{-\infty}^{\infty}\delta(\psi,\psi^*)\phi\left(\sqrt{\tau_\varepsilon+\tau_\psi}\left(\psi-\frac{P_0\tau_\varepsilon+\mu_x\tau_\psi}{\tau_\varepsilon+\tau_\psi}\right)\right)d\psi, \tag{19}$$

where $\delta(\psi, \psi^*)$ is continuous and symmetric with respect to ψ^* with a unique global maximum at $\psi = \psi^*$:

$$\delta(\psi,\psi^*)=\begin{cases} -e^{c^I E[\bar{x}|\psi^*]-E[\bar{x}|\psi]-1/2c_I/Var(\bar{x}|\psi)} & \text{if } \psi<\underline{\psi} \\ -e^{(E[\bar{x}|\psi]-E[\bar{x}|\psi^*])^2/2Var(\bar{x}|\psi)} & \text{if } \underline{\psi}\leq\psi\leq\bar{\psi} \\ -e^{c^I E[\bar{x}|\psi]-E[\bar{x}|\psi^*]-1/2c_I/Var(\bar{x}|\psi)} & \text{if } \bar{\psi}<\psi \end{cases}$$

It follows that the value of ψ^* that maximizes (19) equals $\frac{P_0\tau_\varepsilon+\mu_x\tau_\psi}{\tau_\varepsilon+\tau_\psi}$. Solving (16) for $\psi^* = \frac{P_0\tau_\varepsilon+\mu_x\tau_\psi}{\tau_\varepsilon+\tau_\psi}$ yields $D_0^I=D_0^U=\mu_s$.

Analyst's Forecasting Strategy

We start by solving for the equilibrium-forecasting strategy and the investor's trade for the case where $\psi>\psi^*+c_I$ and showing its uniqueness. For $\psi>\psi^*+c_I$, the analyst maximizes:

$$c_A\frac{E[\bar{x}|\hat{\psi}(x^R)]-P_0-c_I}{\rho Var(\bar{x}|\psi)}-\frac{1}{2}E[(x^R-\bar{x})^2|\psi],$$

where $\hat{\psi}(x^R)$ denotes the informed investor's beliefs about the analyst's private signal Ψ following the forecast x^R. The FOC is:

$$0=c_A\frac{\hat{\psi}'(x^R)}{\rho Var(\bar{x}|\psi)}-E[x^R-\bar{x}|\psi]$$

$$\Leftrightarrow 0=c_A\frac{\hat{\psi}'(x^R)}{\rho Var(\bar{x}|\psi)}-b(\psi). \tag{20}$$

In equilibrium, the informed investor's beliefs about the analyst's forecasting strategy has to be correct—i.e., $\hat{\psi}(x^R(\psi))=\psi$. Then, $\hat{\psi}'(x^R)=1/\partial x^R(\psi)/\partial\psi=1/1+b'(\psi)$. With $Var(\bar{x}|\psi)=1/\tau_\varepsilon$, we can rewrite (20) as:

$$0=\frac{c_A}{\rho}\frac{\tau_\varepsilon}{1+b'(\psi)}-b(\psi)$$

$$\Leftrightarrow 0=b(\psi)+b'(\psi)b(\psi)-\frac{c_A\tau_\varepsilon}{\rho}.$$

Hence, the FOC is a differential equation for $b(\Psi)$. With the boundary condition that $b(\psi^*+c_I)=0$, the Fundamental Theorem of Differential Equations guarantees that the solution exists and is unique. The unique solution is:

$$b(\psi)=\frac{c_A\tau_\varepsilon}{\rho}(1+PL(-e^{-(\psi-c_I-\psi^*)\rho/c_A^2\tau_\varepsilon-1})) \text{ for } \psi>\psi^*+c_I, \tag{21}$$

where $PL()$ denotes the *ProductLog* function (see below).

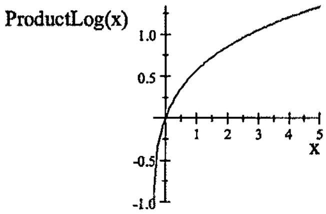
For $\psi<\psi^*-c_I$ the differential equation for $b(\Psi)$ is:

$$0=b(\psi)+b'(\psi)b(\psi)+\frac{c_A\tau_\varepsilon}{\rho}.$$

The boundary condition for $\psi<\psi^*-c_I$ is $b(\psi^*-c_I)=0$ and, hence:

$$b(\psi)=-\frac{c_A\tau_\varepsilon}{\rho}(1+PL(-e^{-(\psi^*-\psi)\rho/c_A\tau_\varepsilon-1})) \text{ for } \psi<\psi^*-c_I.$$

FIGURE 4
ProductLog(x)



ProductLog(x)

The *ProductLog*(x) function (also known as Lambert-W or Omega function) is the solution to the differential equation $f'(x) = \frac{f(x)}{x(1+f(x))}$. It is defined as any $x \geq -e^{-1}$, where *ProductLog*$(-e^{-1}) = -1$, *ProductLog*$(0) = 0$, and *ProductLog*$(x) = \infty$. Figure 4 demonstrates the *ProductLog*(x) function.

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Earnings Quality and International IPO Underpricing

Thomas J. Boulton
Miami University

Scott B. Smart
Indiana University

Chad J. Zutter
University of Pittsburgh

ABSTRACT: This study examines the impact of country-level earnings quality on IPO underpricing. Examining 10,783 IPOs from 37 countries, we find that IPOs are underpriced less in countries where public firms produce higher quality earnings information. This finding persists after controlling for other deal- and country-specific factors that affect IPO underpricing, and it is driven neither by the large and relatively transparent markets in the U.S. and U.K. nor by the relatively opaque Japanese market. The impact of low earnings quality on underpricing is partially offset by the use of a top-tier underwriter.

Keywords: *international finance; earnings quality; initial public offerings; underpricing.*

Data Availability: *Data used in this study are available from public sources.*

JEL Classifications: *G15; G24; G30; G32; G34.*

I. INTRODUCTION

Few corporate events garner more attention from researchers, practitioners, the media, and the public than initial public offerings (IPOs). Generally, the focus is on the large, sometimes spectacular, first-day gains to new issues observed not only in the U.S., but also in virtually all of the world's stock markets. Summarizing the findings of dozens of studies, the

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majority of which focus on underpricing in a single country, Loughran et al. (1994) confirm that IPOs earn positive first-day returns everywhere, and that underpricing varies dramatically across countries. However, what drives underpricing differences across markets is still a largely unexplored question.

Theoretical explanations of the underpricing phenomenon often propose that underpricing arises from information asymmetries among participants in the IPO process. To cite just one prominent example, Rock (1986) shows that when investors have different information sets, underpricing is necessary to induce less informed investors to bid for IPO shares in equilibrium.[1] Asymmetric information theories receive considerable empirical support. In a recent survey of the empirical evidence, Ljungqvist (2007, 380) concludes that information asymmetries "have a first-order effect on underpricing." We investigate whether some of the cross-country underpricing variation documented in the literature may be driven by differences in the quality of information available to investors in different markets. Specifically, we are interested in whether international differences in the quality of accounting information (i.e., earnings quality) help explain the international cross-section of IPO initial returns (i.e., IPO underpricing).

We draw upon the extant literature for several proxies for earnings quality and to determine whether those proxies influence underpricing across firms in different countries. There is ample evidence in both the accounting and finance literatures that the earnings quality measures in our tests are correlated with a wide range of capital market outcomes.

For example, Bhattacharya et al. (2003) find that, in countries with greater earnings opacity, firms face a higher cost of capital. Biddle and Hilary (2006) establish that better accounting information leads to more efficient firm-level investment decisions, particularly in countries where financing comes through arm's-length transactions rather than through relationships with creditors. Gelos and Wei (2005) find that emerging markets mutual fund managers invest more heavily in countries with greater transparency. Jin and Myers (2006) argue that greater opacity leads to lower firm-specific risk borne by investors and more synchronous stock prices, indicating that the relative amount of firm-level information (as opposed to market-level information) that is capitalized into stock prices is lower in more opaque countries.

These findings, combined with the central role that information asymmetries play in IPO underpricing, give rise to our earnings quality hypothesis:

> **Hypothesis:** Do firms endure higher underpricing when they go public in countries where the quality of earnings information is relatively low?

We approach this question in an international setting for several reasons. First, as noted above, underpricing varies dramatically across countries, though relatively little work has been done to understand why this is the case. Second, within a single country, firms operate in the same legal and regulatory environment, and that environment likely imposes some limit on the variability of earnings quality across firms within a country. Thus, it is plausible that the quality of accounting information varies more across markets than within a single market. Third, in most cases, very little accounting information is available about a particular firm prior to its IPO. An IPO prospectus typically offers little more than two years of pre-IPO financial information. This limits the ability to capture cross-sectional earnings quality differences as of the IPO date. In contrast, the country-level earnings quality measures that we use draw upon several years of data generated by many different firms. While these measures do not provide specific information on the quality of earnings from one IPO to another in a given country, they do capture differences in the information environment in which firms from different countries go public. Fourth, we are interested in whether reputable financial intermediaries, such as top-tier investment banks, play a role in mitigating the information asymmetries related to earnings quality. Given the global scope of the investment banking industry, and given the difficulties in identifying cross-sectional earnings quality variance in a single country, the question of whether investment banks mitigate earnings-related information asymmetries seems ideally suited to an international sample.

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[1] Other information-based underpricing theories endow different participants in the IPO process with superior information. For example, in Baron (1982) investment bankers know more about an IPO firm's true value than does the issuer, and underpricing becomes a solution to the resulting principal-agent problem. In Allen and Faulhaber (1989), Grinblatt and Hwang (1989), and Welch (1989) issuers use their information advantage to signal firm quality, whereas in Benveniste and Spindt (1989) underpricing induces well-informed investors to reveal what they know before the offer price is set.

Although the advantages of studying underpricing using an international sample are numerous, we must mention two caveats associated with our research design. First, underpricing differences across countries could be influenced by omitted variables, such as differences in offering methods, which could be correlated with our earnings quality measures. We have taken many steps to minimize this possibility, for example by controlling for country and deal characteristics such as the offering method. In addition, we appeal to evidence that IPO pricing methods around the world are converging over time, with the U.S. bookbuilding approach dominating in most countries. For instance, Ljungqvist et al. (2003) find that, in a sample of 2,143 IPOs from 65 countries, 46.2 percent were priced using the bookbuilding method in 1994, but by the end of their sample period (the first seven months of 1999), the fraction of new issues priced via bookbuilding rose to 80 percent. Similarly, Jagannathan and Sherman (2006) examine the use of auctions and bookbuilding in 46 countries and find that in all but four countries, auctions have been abandoned entirely and are rare in the few countries that still use them. In contrast, they find that the bookbuilding method has been gaining market share over time and has become the dominant pricing method in most countries. While we cannot rule out the possibility of an omitted variables problem in our analysis, the timeliness of our sample (1998–2008) combined with our regression control variables and robustness tests reduce this problem to the extent possible.

The second caveat is that private firms can presumably mitigate asymmetric information problems by providing high-quality accounting information leading up to their IPOs, but we do not have data on pre-IPO disclosures of private firms. Our country-level measures are intended to quantify, in a broad sense, the quality of information generally provided by public firms in a given country, but these measures may or may not represent a good characterization of the reporting practices of private firms prior to their IPOs. Ball and Shivakumar (2005), for example, find that the reporting done by private U.K. firms more closely resembles the disclosures provided by firms in code-law countries than those of U.K. public companies. Even so, it seems likely that as firms execute their plans to go public, they adapt their reporting practices to align with those of existing public firms. In that case, IPO firms would not resemble other private firms in terms of their reporting strategies. In either case, there may be limits to the benefits that a given firm can capture by adopting reporting practices superior to those that are common in its home country. For example, Doidge et al. (2007) find that firms in countries where corporate governance practices are relatively poor find it difficult to credibly commit themselves to better governance.

Examining 10,783 IPO events, we find evidence of a statistically and economically significant association between country-level earnings quality and IPO-firm-level underpricing. Comparing two countries that differ in earnings quality by one standard deviation reveals that firms going public in the country with better earnings information experience 7.8 percentage points lower underpricing (the sample mean initial return is 36.5 percent).[2] Our regression models control for many deal-specific (e.g., offer size, industry, underwriter, and underwriting method) and country-specific (e.g., market returns, liquidity, and IPO activity) variables that influence underpricing, and our results are robust to accounting for price-stabilization activities and various minimum offer price screens. Furthermore, the link between earnings quality and underpricing is driven neither by the large and relatively transparent U.S. and U.K. markets nor by the relatively opaque market in Japan. Finally, we find evidence that top-tier underwriters mitigate the effect of poor country-level earnings quality on IPO underpricing, such that firms listing in countries with relatively low earnings quality experience a marginal reduction in underpricing if they are backed by top-tier underwriters.

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[2] This statement is based on the standard deviation of aggregate EM reported in Table 3 and the regression estimate of aggregate EM reported in Table 5.

Section II highlights previous research on earnings quality and IPOs related to this study. Section III describes our sample construction and descriptive statistics. Section IV contains our primary results on the relations between the quality of earnings and IPO underpricing and illustrates that many of the conventional variables used in single-country studies of IPO underpricing also explain the international cross-section. Section V summarizes and concludes.

II. EARNINGS QUALITY AND IPOS

A number of researchers have studied the link between accounting information disclosed in the IPO prospectus and the market value of going-public firms. Essentially, this strain of the literature asks whether IPO firms manipulate their financial statements to obtain a higher share price. Early studies offer modest affirmation for this hypothesis (e.g., Clarkson et al. 1992; Aharony et al. 1993; Friedlan 1994). Teoh et al. (1998b) report that IPO firms have high issue-year earnings and abnormal accruals, followed by poor long-run earnings and negative abnormal accruals and that abnormal accruals at the IPO help explain subsequent poor stock returns. Similarly, Teoh et al. (1998a) find that IPO firms that are the most aggressive in using accruals to report cash flows in excess of earnings earn 20 percent lower stock returns in the three years after the IPO compared to the firms reporting the most conservative earnings figures.[3] However, Fan (2007) reports that discretionary accruals are indeed highest in the IPO year, consistent with the notion that IPO issuers manage earnings, but she finds no evidence of a negative relation between IPO earnings management and subsequent stock returns. Further, Venkataraman et al. (2008) examine pre-IPO financial statements and find that pre-IPO accruals tend to be negative and less than post-IPO accruals. In the same vein, Ball and Shivakumar (2008) find that firms' reporting practices become more conservative when they transition from private to public status through an IPO.

All of these studies focus on the extent to which earnings manipulation leading up to the IPO influence the firm's stock price. They do not look at underpricing (i.e., the difference between the IPO offer price and the market price established once trading begins), which is our focus. The accounting literature offers several insights regarding the influence of accounting disclosures on IPO underpricing.

In a study of micro-cap Nasdaq IPOs, Willenborg and McKeown (2001) find that firms going public with going-concern audit opinions are more likely to delist within two years of the IPO, but these firms also endure less underpricing, consistent with the notion that the audit opinion reduces information asymmetries between issuers and investors. Jog and McConomy (2003) examine the impact of voluntary disclosure of management earnings forecasts in the IPO prospectus on IPO valuation and performance. They find higher underpricing for IPOs that do not include earnings forecasts, though this difference is concentrated among small firms. Schrand and Verrecchia (2005) study the relation between underpricing and the frequency of pre-IPO disclosure, finding lower underpricing for firms with more frequent disclosures prior to the IPO. An exception is Internet firms, where this relation is reversed. They also find that more frequent disclosure ameliorates adverse selection in the aftermarket, with lower bid-ask spreads and greater depths for firms that disclose more frequently pre-IPO. Finally, Leone et al. (2007) find that IPO underpricing decreases when issuers disclose more specific information in the "uses of proceeds" section of the prospectus. Collectively, these studies, all of which focus on IPOs in a single country, suggest that accounting disclosures influence underpricing.

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[3] Aharony et al. (2000), DuCharme et al. (2001), and Teoh and Wong (2002) also find evidence of pre-IPO earnings manipulation.

Our study extends this analysis to a multi-country setting to determine if earnings quality at the country level influences underpricing costs borne by firms going public in different markets. In so doing, we add to the very limited evidence on the determinants of cross-country underpricing variation as well as contribute to another strain of the literature on the quality and value of accounting information in different countries. For example, DeFond et al. (2007) find that earnings announcements are more informative in countries with better overall earnings quality. Bhattacharya et al. (2003) study the extent to which cross-country variation in the quality of accounting information influences the cost of capital and trading volume in international equity markets. Using data from 34 countries covering 1985–1998, they measure three dimensions of earnings opacity for each country—earnings aggressiveness, loss avoidance, and earnings smoothing. They find robust evidence that an increase in overall earnings opacity leads to an increase in the required return demanded by shareholders and a decrease in trading activity. In a similar vein, Leuz et al. (2003) examine earnings management practices in 31 countries and find that firms engage in greater earnings management in countries with weaker investor protections.[4]

These studies and others suggest that the quality of earnings information available to outside investors influences information asymmetries in the market. Given the central role of asymmetric information in theories of IPO underpricing, we anticipate that differences in earnings quality across countries will influence the underpricing costs that firms going public in different countries face. Recognizing that earnings quality is a multi-faceted concept and that no single measure can capture all aspects of earnings quality, we employ a wide range of earnings quality measures drawn from the extant literature and study their association with firm-level underpricing. The measures we use are designed to capture elements of earnings quality such as earnings smoothing, loss avoidance, and earnings aggressiveness, among others. Below, we construct and use the earnings quality measures in Leuz et al. (2003) and Bhattacharya et al. (2003) to test our earnings quality hypothesis.

III. SAMPLE CONSTRUCTION AND DESCRIPTIVE STATISTICS

Sample Construction

We begin our sample construction by retrieving each IPO with a valid offer price and offer size for all countries reported in the Thomson Financial SDC Platinum New Issues database from 1998 through 2008. We exclude financial firms, rights offerings, unit offerings, closed-end funds, trusts, limited partnerships, and depository receipts. This initial extraction from SDC results in 13,390 IPOs from around the world. Next we attempt to match each IPO firm with the Datastream database to collect the secondary market prices that we need to calculate IPO initial returns. This matching process is done in two steps: (1) we use the SEDOL firm identifier included in both the SDC and Datastream databases, and (2) we hand-match any firm that does not have a SEDOL reported in both databases. We are able to match 74.4 percent of the SDC IPOs using the SEDOL and an additional 16.2 percent by hand, for a total of 12,122 IPOs matched with Datastream.

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[4] Bushman and Piotroski (2006) construct earnings quality measures based upon accounting conservatism and link those measures to cross-country variation in legal and political institutions. See also Hung (2000) and Ball et al. (2003).

We next drop 950 IPOs that do not have a valid "first-day" secondary market closing price in Datastream. We define a valid secondary market closing price as a price observation in Datastream with positive trading volume (i.e., a transaction price). The first-day secondary market closing price is the first valid closing price that occurs within −3 to +60 days of the SDC IPO issue date.[5] From the remaining 11,172 IPOs, we drop an additional 161 deals due to incomplete or missing data for our earnings quality measures or control variables.

For 11,011 IPOs from around the world, we calculate the initial return as the first-day secondary market closing price divided by the IPO offer price, minus 1.[6] To eliminate the impact of outliers, we trim our sample by removing the top and bottom 1 percent based on initial returns.[7] Finally, we exclude countries with fewer than five IPOs during our sample period. These steps result in a final sample of 10,783 IPOs listed in 37 countries. Table 1 summarizes the sample selection process.

Although common in the IPO literature, we do not impose a minimum offer price restriction. For example, Ritter (1991) evaluates U.S. IPOs with a minimum offer price of $1 to mitigate the bid-ask bounce effect. However, imposing this filter would not only greatly reduce the number of IPOs in many countries, but it would also eliminate some countries entirely. Applying a $1 minimum offer price (converting local currency to U.S. dollars based on the exchange rate as of the IPO date) eliminates over one-third of the sample events. Thus, the main analysis presented here imposes no minimum offer price, but we do verify that our results are unaffected by the inclusion of IPOs with very low offer prices.

TABLE 1
Sample Construction

IPO Sample Selection Procedure	Number of IPOs Dropped	Remaining IPOs
(1) Pull IPOs for all countries from SDC New Issues database.		13,390
(2) Drop IPOs that cannot be matched with the Datastream database.	1,268	12,122
(3) Drop IPOs that do not have a valid first-day secondary market closing price.	950	11,172
(4) Drop IPOs from countries not covered by LLSV antidirector rights index (except for IPOs from China).	140	11,032
(5) Drop IPOs from countries for which earnings quality measures cannot be calculated for the five years prior to the IPO.	21	11,011
(6) Drop IPOs with an initial return in the 1st or 99th percentile.	223	10,788
(7) Drop IPOs from countries with less than five IPOs.	5	10,783

This table presents the sample construction process for the entire sample of 10,783 IPOs listed in 37 countries.

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[5] In 39 cases, representing less than 0.4 percent of our sample, there is an apparent error in the SDC IPO date because the first closing price of the firm found in Datastream occurs one, two, or three days prior to the SDC IPO date. For France, Greece, and Taiwan we use the tenth valid price, rather than the first, because in these countries secondary prices are initially constrained by daily volatility limits for a few days following the IPO.

[6] When calculating initial returns, closing and offer prices are in the IPO firm's local currency.

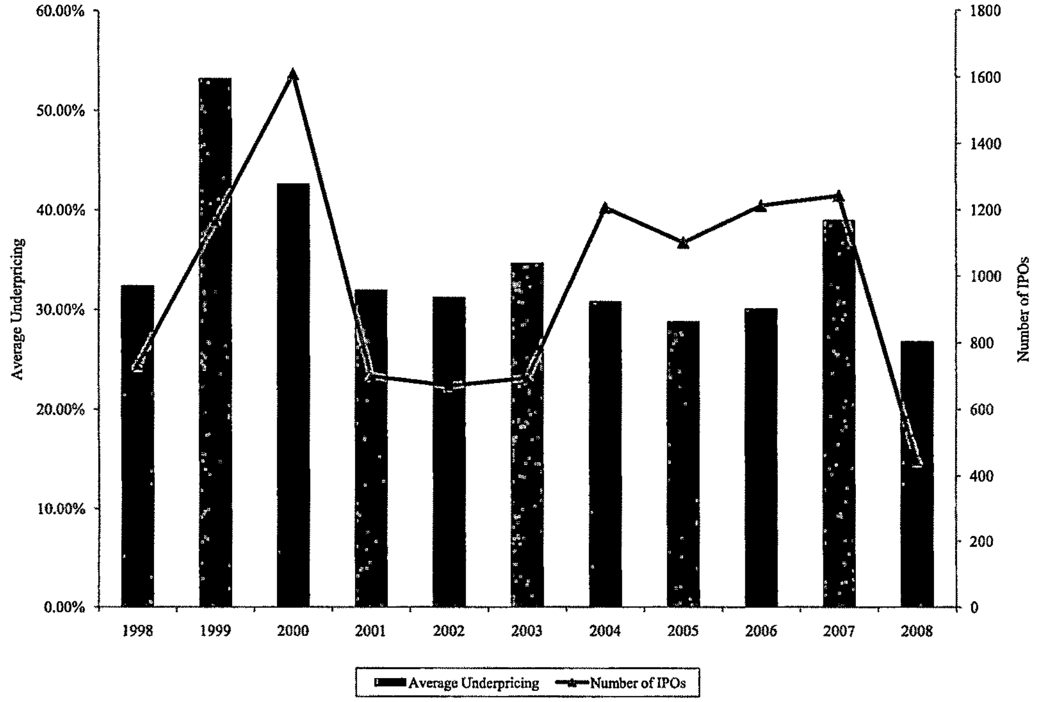
[7] Initial returns at the 1st and 99th percentiles cutoffs are −36 and 388 percent, respectively.

Descriptive Statistics

Figure 1 displays the average IPO underpricing and number of IPOs by year. The figure clearly illustrates the slowdown in equity offerings following the decline in equity markets in 2000. IPO volume falls more than 56 percent from 2000 to 2001, then remains relatively flat for 2002 and 2003. The number of new issues rebounds sharply to 1,206 deals in 2004 and remains above 1,100 issues per year through 2007. The worldwide financial crisis of 2008 had a significant negative impact on IPO issuance worldwide, with only 444 firms going public. Consistent with the "hot issues market" phenomenon (e.g., Ritter 1984), average underpricing is positively correlated with yearly IPO volume (correlation = 0.51).

Coffee (1999, 2002) suggests that firms list abroad to bond themselves to foreign listing standards. For example, firms listing in the U.S. subject themselves to SEC oversight, agree to meet generally accepted accounting principles (GAAP), and face the scrutiny of financial intermediaries involved in the security markets. While most of our IPOs originate and list in the same country, some companies choose to list outside their home country. We exclude depositary receipts from our sample, but retain firms that list their shares directly in a foreign market. Most of these

FIGURE 1
Average Underpricing and Number of IPOs by Year



Bar heights show the average underpricing by year. Line points show the number of IPOs by year.

firms list in the U.S. or the U.K. Because listing abroad can bond management to the listing country's standards, the country where the firm lists is the relevant location for this study.[8]

Table 2 shows the IPO volume, aggregate gross IPO proceeds, average underpricing, and the average value of each earnings quality variable for each country in our sample. Not surprisingly, the U.S. has the most IPOs in the sample, followed closely by Japan, the U.K., and Australia. The aggregate gross proceeds for the entire sample is about $1.12 trillion, of which the U.S. represents about 29 percent. Table 2 confirms that IPO firms are underpriced, on average, in every country, and that average underpricing varies widely across countries. First-day returns range from 120.7 percent in China to less than 2 percent in Argentina.

For each country and each year, we calculate several earnings quality measures, described in more detail in the next section. One such measure is a country's aggregate earnings management (aggregate EM) score, which is constructed such that a higher score implies greater earnings management, and therefore lower quality earnings. The average aggregate EM scores indicate that the countries with the least (most) earnings management are Australia, South Africa, and the U.S. (China, Indonesia, and Taiwan). An alternative earnings quality measure is earnings opacity. The average earnings opacity scores indicate that the countries with the most transparent (opaque) earnings are Norway, the U.S., and Canada (China, India, and Hong Kong).[9] At the bottom of Table 2 we report the simple correlations between the average IPO underpricing and earnings quality measures. The positive correlations are consistent with our earnings quality hypothesis, which predicts greater underpricing in countries with lower quality earnings.

Figure 2 offers preliminary evidence that earnings quality influences underpricing. Figure 2 groups countries into earnings quality quartiles based on their average yearly aggregate earnings management score for the sample period. Countries with the highest aggregate EM are in the lowest earnings quality quartile and those with the lowest aggregate EM are in the highest earnings quality quartile. Figure 2 reports the average underpricing and average aggregate EM across IPOs within an earnings quality quartile. Average underpricing is almost 77 percent higher (or over 21 percentage points higher) in the lowest earnings quality quartile compared to the highest quartile. Across earnings quality quartiles, the simple correlation between average IPO underpricing and average aggregate EM is 76 percent. Of course, many differences in the IPO process and in the types of firms going public exist across countries, so in the next section we test for a link between earnings quality and IPO underpricing while holding constant deal-level and other country-level characteristics.

Table 3 provides descriptive statistics for the variables used in our regression analysis, starting with various earnings quality measures drawn from Leuz et al. (2003) and Bhattacharya et al. (2003). For these measures, we provide up-to-date, yearly calculations (described below) for each of our sample countries and scale these measures such that a higher value indicates lower earnings quality.

The unit of observation in our regressions is the IPO firm, and the dependent variable is the first-day return. The sample mean initial return equals 36.5 percent, which is roughly double the 18.7 percent mean reported by Loughran and Ritter (2004) for 6,391 IPOs issued in the U.S. between 1980 and 2003. The cross-sectional standard deviation of initial returns is 61.2 percent, indicating large variations in IPO returns. Unquestionably, some of this variation can be explained by deal-specific and market-specific factors not related to earnings quality, so we use the remaining variables listed in Table 3 to control for these effects.

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[8] Unreported robustness tests confirm that our results are not sensitive to the exclusion of firms that choose to list in a foreign country.

[9] To provide a check of our earnings quality variables against those published in other studies, we compared our calculations to those published in Bhattacharya et al. (2003, Table 1). For comparable variables, the correlation between our measures and theirs (which were taken from a slightly different time period) is 0.87.

TABLE 2
Country-Level Descriptive Statistics

Country	n	Aggregate Gross Proceeds (U.S.$ MM)	Average Underpricing	Average EM1	Average EM2	Average EM3	Average EM4	Average Aggregate EM	Average Earnings Aggressiveness	Average Earnings Opacity
Argentina	5	247.73	1.43%	−0.73	0.78	0.48	0.89	14.60	−0.06	3.88
Australia	1,009	26,659.45	23.06%	−0.74	0.81	0.47	0.62	7.46	−0.04	4.10
Austria	31	7,278.92	6.10%	−0.32	0.94	0.69	0.75	31.44	−0.06	4.73
Belgium	61	14,515.28	10.19%	−0.37	0.93	0.66	0.78	29.75	−0.07	4.58
Brazil	46	16,982.60	6.17%	−0.57	0.90	0.51	0.67	14.17	−0.02	5.99
Canada	421	13,138.84	48.61%	−0.60	0.88	0.51	0.66	14.13	−0.05	3.86
China	686	58,730.82	120.72%	−0.37	0.96	0.70	0.97	35.87	−0.01	8.03
Denmark	27	2,341.67	7.09%	−0.40	0.89	0.57	0.76	24.12	−0.04	4.65
Finland	43	6,063.64	26.14%	−0.47	0.91	0.46	0.94	24.01	−0.04	5.57
France	495	71,490.92	15.84%	−0.39	0.97	0.58	0.76	28.55	−0.04	5.54
Germany	366	59,422.92	36.63%	−0.43	0.95	0.64	0.79	27.68	−0.06	4.97
Greece	109	5,272.99	58.33%	−0.29	0.90	0.63	0.93	33.17	0.01	6.00
Hong Kong	583	79,494.15	16.69%	−0.36	0.96	0.75	0.74	33.08	−0.02	6.60
India	169	16,974.68	40.21%	−0.47	0.98	0.45	0.83	26.71	−0.02	7.58
Indonesia	60	5,260.06	39.32%	−0.31	0.95	0.83	0.75	35.26	−0.04	5.64
Ireland	7	2,735.77	3.85%	−0.63	0.95	0.42	0.84	19.75	−0.03	6.03
Israel	9	251.67	24.28%	−0.40	0.94	0.70	0.78	29.94	−0.03	5.58
Italy	156	50,044.99	10.46%	−0.28	0.93	0.67	0.77	32.13	−0.04	5.49
Japan	1,259	85,327.98	55.85%	−0.37	0.94	0.63	0.79	30.37	−0.03	6.40
Malaysia	309	5,944.56	34.32%	−0.44	0.91	0.78	0.74	28.51	−0.02	6.27
Mexico	11	3,113.55	7.69%	−0.46	0.84	0.43	0.71	15.61	−0.03	5.30
The Netherlands	46	14,118.38	28.48%	−0.42	0.92	0.51	0.74	22.67	−0.05	4.60
New Zealand	36	2,191.18	10.45%	−0.57	0.60	0.39	0.69	9.53	−0.04	4.71
Norway	96	14,112.62	3.74%	−0.59	0.32	0.66	0.60	13.50	−0.06	3.30
Philippines	22	1,831.36	12.32%	−0.43	0.84	0.76	0.69	27.59	−0.04	5.19
Portugal	10	6,242.68	15.95%	−0.28	0.94	0.66	0.80	33.88	−0.05	5.30
Singapore	462	14,389.05	27.08%	−0.39	0.96	0.80	0.71	32.22	−0.03	6.18

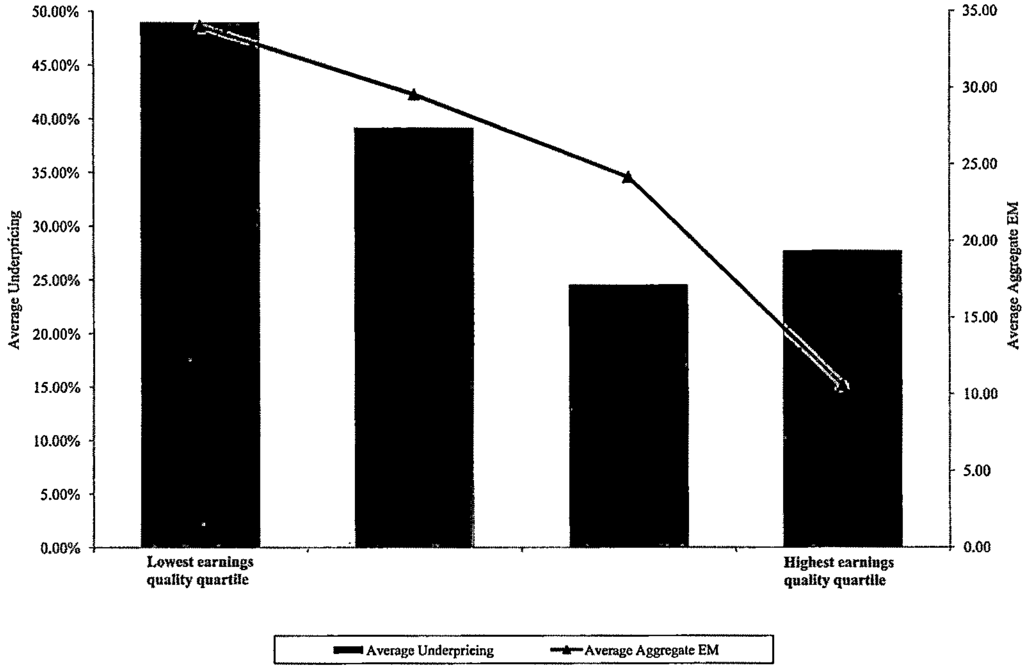
(continued on next page)

TABLE 2 (continued)

Country	n	Aggregate Gross Proceeds (U.S.$ MM)	Average Underpricing	Average EM1	Average EM2	Average EM3	Average EM4	Average Aggregate EM	Average Earnings Aggressiveness	Average Earnings Opacity
South Africa	8	1,082.68	8.53%	−0.59	0.79	0.42	0.53	7.94	−0.04	4.23
South Korea	517	21,252.00	46.63%	−0.37	0.92	0.64	0.75	28.04	−0.04	5.70
Spain	38	27,860.74	14.76%	−0.36	0.97	0.54	0.86	30.64	−0.04	6.15
Sweden	76	17,718.82	9.21%	−0.60	0.90	0.52	0.65	14.33	−0.04	4.91
Switzerland	69	24,351.77	11.49%	−0.41	0.93	0.52	0.83	25.81	−0.05	5.46
Taiwan	547	10,382.86	22.96%	−0.36	0.97	0.79	0.75	34.01	−0.03	6.49
Thailand	142	11,492.59	17.31%	−0.41	1.00	0.65	0.74	31.96	−0.05	5.95
Turkey	16	3,870.52	7.22%	−0.69	0.84	0.67	0.80	20.83	0.03	5.64
U.K.	1,044	91,006.69	17.69%	−0.59	0.88	0.49	0.69	14.92	−0.05	4.52
U.S.	1,792	325,125.95	33.90%	−0.57	0.68	0.46	0.63	8.67	−0.04	3.63
Correlation with average underpricing				0.30	0.25	0.23	0.40	0.36	0.31	0.49
p-value				(0.075)	(0.139)	(0.177)	(0.014)	(0.030)	(0.060)	(0.002)

This table presents country-level descriptive statistics for the entire sample of 10,783 IPOs. n is the number of sample IPOs listing in the country over the 1998–2008 sample period. Aggregate gross proceeds is the sum of CPI-adjusted offer value in 2008 U.S. dollars for the country of listing. Underpricing is the first-day secondary market closing price divided by the final offer price, minus 1. EM1 is the median ratio in country *i* of the firm-level standard deviations of operating earnings over the cash flow from operations (both scaled by lagged total assets), multiplied by −1. EM2 is the cross-sectional correlation in country *i* between the change in accruals and change in cash flows from operations (both scaled by lagged total assets), multiplied by −1. EM3 is the median ratio in country *i* of the absolute value of accruals over the absolute value of cash flow from operations. EM4 is the ratio in country *i* of the number of firms reporting small profits over the sum of the number of firms reporting small losses and small profits. A small profit (loss) is defined as a value of net earnings scaled by lagged total assets in the range [0, 0.01] ([−0.01, 0)). Aggregate EM is the average country *i* ranking across the following four earnings management measures: EM1, EM2, EM3, and EM4. Earnings aggressiveness is the median ratio in country *i* of total accruals over the lagged total assets. Earnings opacity is the average country *i* decile ranking across the following three earnings management measures: EM2, EM4, and earnings aggressiveness. The final two rows report the correlation between the average earnings quality measure and average underpricing and the p-value from a test of the null hypothesis that the correlation is equal to 0.

FIGURE 2
Average Underpricing and Average Aggregate EM by Earnings Quality Quartiles



Sample countries are sorted into quartiles based on their average aggregate EM score over the 1998–2008 sample period. Countries with the highest aggregate earnings management are in the lowest earnings quality quartile and those with the lowest aggregate earnings management are in the highest earnings quality quartile. Bar heights reflect the average underpricing across all IPOs within a quartile. Line points reflect the average aggregate EM score across all IPOs within a quartile.

Because the extent to which an IPO is underpriced may be influenced by the quality of the underwriter, we create an indicator variable to identify top-tier underwriters. Underwriters listed in the top 25 of SDC's global league tables for the issue year are coded as top-tier underwriters. Table 3 indicates that 22.5 percent of our IPOs employ a top-tier underwriter.[10]

In some countries, underwriters can engage in price support once trading begins. Underwriters may have incentives to engage in price stabilization, particularly when an IPO's market price begins to fall below the IPO offer price. If underwriters engage in price support to limit the occurrence of negative initial returns, then the first-day returns distribution will have a higher mean than it would have in the absence of price support. We do not have detailed data on the regulations and practices with respect to price stabilization in all countries, so we attempt to control for price-stabilization activity in two ways. First, we construct a country-level proxy for

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[10] In unreported tests, we controlled for underwriter reputation using country-level or global market share measures. These measures are consistent with Megginson and Weiss (1991), who use market share to proxy for underwriter reputation. Our results are broadly similar when these measures are used to proxy for underwriter reputation.

TABLE 3
IPO Sample Descriptive Statistics

Variable	n	Mean	Std. Dev.	Minimum	Maximum
EM1	10,783	−0.480	0.139	−0.844	−0.173
EM2	10,783	0.876	0.150	−0.402	1.000
EM3	10,783	0.592	0.131	0.275	0.996
EM4	10,783	0.728	0.103	0.367	1.000
Aggregate EM	10,783	22.388	11.145	4.750	45.000
Earnings aggressiveness	10,783	−0.036	0.015	−0.100	0.206
Earnings opacity	10,783	5.326	1.392	2.867	8.667
Initial return	10,783	0.365	0.612	−0.360	3.862
Top-tier underwriter	10,783	0.225	0.418	0.000	1.000
Price stabilization	10,783	0.007	0.019	−0.074	0.125
IPO activity	10,783	0.045	0.029	0.001	0.146
Market return	10,783	0.030	0.112	−0.488	1.132
Stock market turnover ratio	10,783	1.109	0.638	0.074	2.946
Index of economic freedom	10,783	72.568	9.738	44.858	91.367
Antidirector rights index	10,783	3.834	1.199	0.000	5.000
Offer size ratio	10,783	1.000	4.861	0.000	351.819
Stock return volatility	10,708	0.048	0.040	0.000	1.571
Integer offer price	10,783	0.500	0.500	0.000	1.000
Bookbuilt	10,198	0.612	0.487	0.000	1.000
Firm commitment	10,707	0.560	0.496	0.000	1.000
Equity carve-out	10,699	0.034	0.181	0.000	1.000

This table presents descriptive statistics for the entire sample of 10,783 IPOs. EM1 is the median ratio in country *i* of the firm-level standard deviations of operating earnings over the cash flow from operations (both scaled by lagged total assets) multiplied by −1. EM2 is the cross-sectional correlation in country *i* between the change in accruals and change in cash flows from operations (both scaled by lagged total assets) multiplied by −1. EM3 is the median ratio in country *i* of the absolute value of accruals over the absolute value of cash flow from operations. EM4 is the ratio in country *i* of the number of firms reporting small profits over the sum of the number of firms reporting small losses and small profits. A small profit (loss) is defined as a value of net earnings scaled by lagged total assets in the range [0, 0.01] ([−0.01, 0)). Aggregate EM is the average country *i* ranking across the following four earnings management measures: EM1, EM2, EM3, and EM4. Earnings aggressiveness is the median ratio in country *i* of total accruals over the lagged total assets. Earnings opacity is the average country *i* decile ranking across the following three earnings management measures: EM2, EM4, and earnings aggressiveness. Initial return is the secondary market closing price divided by the final offer price minus 1. Top-tier is an indicator variable set to 1 for IPOs underwritten by an investment bank appearing in the top 25 of SDC's league tables in the issue year, and 0 otherwise. Price stabilization is the difference in the number of IPOs with small positive first-day returns (greater than 0 and less than or equal to 1 percent) and the number of IPOs with small negative first-day returns (less than 0 and greater than or equal to −1 percent) divided by the total number of IPOs issued in the country of listing. IPO activity is the ratio of the total number of IPOs in the issue year divided by the number of Datastream-listed equities for the country of listing as of 2008. Market return is the return on the Datastream index for the country of listing over the three months preceding the offering. Stock market turnover ratio equals the ratio of the total value of shares traded to aggregate market capitalization and reported in Beck et al. (2000). Index of economic freedom is a product of The Heritage Foundation and the *Wall Street Journal* and is an aggregate measure covering the following ten freedoms: business, trade, monetary, freedom from government, fiscal, property rights, investment, financial, freedom from corruption, and labor. Antidirector rights index measures shareholder rights by considering the following issues: (1) vote by mail, (2) shares not blocked or deposited prior to shareholder meetings, (3) cumulative voting in director elections, (4) oppressed minority mechanisms, (5) pre-emptive rights to new issues, and (6) minimum capital requirements and is reported in La Porta et al. (1998) and Allen et al. (2005). Offer size ratio is the CPI-adjusted offer value in millions of U.S. dollars divided by the average of this value across all IPOs in the country of listing. Stock return volatility is the standard deviation of returns over the first 30 calendar days following the offering. Indicator variables are set equal to 1 for integer offer price, bookbuilt, firm commitment, and equity carve-out deals.

price-stabilization activity. If price stabilization is widespread, then we expect to see an unusually large probability mass in the distribution of first-day returns at and just to the right of zero and an unusually small probability mass immediately to the left of zero. Therefore, for each country, we calculate the difference in the number of IPOs with initial returns between 0 and 1 percent and the number of IPOs with initial returns between 0 and -1 percent, and then divide the difference by the total number of IPOs. The more prevalent is price stabilization in a given market the higher should be this ratio, and the higher should be average underpricing. Table 3 reports a mean price stabilization ratio of 0.007, indicating a slightly greater incidence of small positive initial returns than small negative returns. A second approach to control for price stabilization exploits the fact that underwriters typically provide price support for a very limited time. Because stabilization activities are short-lived, the effects of stabilization activities on the IPO returns distribution should diminish over time. In unreported tests we calculate the IPO initial return based on the market price roughly one month (22 trading days) after the first trading day, and test to see if our regression results are robust to this change.

As suggested in numerous studies, including Ritter (1984), underpricing tends to be higher when IPO volume is high and when overall stock market returns are high. We include two variables to control for these "hot market" effects. First, our IPO activity measure equals the number of IPOs in a given country in each year divided by the total number of listed equities in Datastream for that country in 2008. Therefore, this measure takes the same value for all IPOs from a single country in a particular year, but within a country it varies across time, and within a single year it varies across countries. Second, for each IPO, we calculate the return on the Datastream market index in the three months leading up to the offer. Consequently, two firms will share the same market return value only if they go public at the same time in the same country.

Ellul and Pagano (2006) suggest that IPOs in less liquid markets will exhibit larger initial returns. Higher underpricing compensates IPO investors for the illiquidity risk that they bear. To control for differences in liquidity across national markets, we include a country-level stock market turnover ratio, which is defined by Beck et al. (2000) as the ratio of total value of shares traded to market capitalization.[11] Hence, this measure captures a stock market's liquidity relative to its size, and we expect lower initial returns in more liquid markets.

The climate for entrepreneurship varies from one country to another due to a variety of factors such as tax rates, government regulations, corruption, and many others. To control for some of these differences, we include the Index of Economic Freedom, which is produced by the Heritage Foundation and the *Wall Street Journal.* The index is an aggregate of several different measures of economic freedom, including: (1) the ease of starting a new business, (2) the level of trade barriers, (3) the top marginal income tax rate, (4) government spending as a percentage of GDP, (5) the inflation rate, (6) the ease with which foreigners can invest in a given country, (7) the extent of government control of the banking system, (8) legal protection of property rights, (9) corruption, and (10) the ability of a business to hire and fire workers without restrictions.[12] A higher index value indicates greater economic freedom.

Because of the literature spawned by the work of La Porta et al. (2000) illustrating the importance of various investor protections on a range of capital market outcomes, we also attempt to control for variation in governance across our sample countries. The index of economic freedom, given its constituent parts, should provide one measure of control for differences in country-level governance. A second control variable, aimed at holding constant the effects of governance, is the antidirector rights index from La Porta et al. (1998). The antidirector rights index is based on a collection of legal and regulatory variables related to a nation's stance regarding the protection of shareholders. Boulton et al. (2010) report a positive relation between the antidirector rights index and IPO underpricing, which they suggest is a cost incurred by insiders to maintain control in countries with legal systems that offer stronger protections to investors.

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[11] Beck et al. (2000) report the ratio of total value of shares traded to market capitalization for the period 1999–2004. Since our sample period is 1998–2008, we take the average of all years reported for a country by Beck et al. (2000) as our measure of country-level stock market turnover.

[12] Moeller and Schlingemann (2005) use this index to proxy for the restrictiveness of the institutional environment related to corporate acquisitions, and they find that in cross-border acquisitions, bidder returns are higher when targets come from countries with greater freedom.

Most IPO underpricing studies include measures designed to capture information asymmetries, including the natural logarithm of the offer size. Because the amount of money that IPO firms raise varies widely across countries, we also construct an offer size measure that captures the size of an IPO relative to other deals in the same country, which we report in Table 3. This offer size ratio equals the offer size for a particular IPO divided by the mean offer size for IPOs in a given country.[13] We obtain broadly similar results with both absolute and relative deal-size measures, so in Table 3 and in subsequent regressions we simply include the offer size ratio for each IPO. In addition to offer size, we also include a measure of stock return volatility to control for information asymmetry. Our stock return volatility measure is the standard deviation of returns over the first 30 (calendar) days following the IPO.

Bradley et al. (2004) report that IPOs with integer prices experience higher underpricing. To explain this pattern, they propose a "negotiation hypothesis" in which underwriters and issuers bargain over finer offer price increments as the uncertainty surrounding firm value declines. Thus, for IPOs that are particularly difficult to value and, hence, should have higher underpricing, offer prices tend to fall on integers. Fifty percent of our IPOs have an integer price, which compares to 76 percent of U.S. IPOs as reported in Bradley et al. (2004). We include in our regression an indicator variable equal to 1 when the offer price is an integer.[14]

Sherman (2005) notes that methods for taking firms public worldwide are converging toward the traditional U.S. bookbuilding approach, and she argues that bookbuilt offers are associated with lower underpricing than offers conducted via auctions. Sherman (2005) argues that bookbuilding reduces the risk faced by IPO issuers and investors, and therefore should lead to less underpricing. However, she also notes that bookbuilding affords issuers discretion over information expenditures, which she suggests can lead to either more or less underpricing depending on the issuer's preferences. Ritter (1987) provides evidence that firm commitment IPOs are underpriced less than best efforts IPOs. Sixty-one percent of the firms in our sample are taken public through a bookbuilt offering, while 56 percent of the deals are firm commitment offerings. Deals that are both firm commitment and bookbuilt represent 31.6 percent of the sample. We include indicator variables for bookbuilt deals and for firm commitment offerings. Schipper and Smith (1986) and Prezas et al. (2000) provide evidence that equity carve-outs are underpriced less than original IPOs. To capture this effect, we include an indicator variable for carve-outs.

IV. EARNINGS QUALITY

Country-Level Earnings Quality and IPO Underpricing

In this section, we ask whether earnings quality affects underpricing. We calculate two different sets of well-established earnings management measures that are intended to capture the variation in the quality of earnings information across our sample countries. Our premise is that information asymmetry and earnings quality are inversely related. The basic tenet of our earnings quality hypothesis is that managers can take various actions to obscure the true earnings distribution, and that these actions increase information asymmetries and underpricing. To construct our earnings quality measures, we use financial statement data on firms in each country that are already publicly traded. In this way, we attempt to characterize the earnings management practices that are common in each country rather than capturing earnings management behavior for each IPO firm. We use firm-level data from Compustat Global over the years 1993 to 2007 for all nonfinancial firms with a minimum of three consecutive years of accounting data. Each country included in the sample has a minimum of 200 firm-year observations from Compustat Global. Each earnings management measure is constructed annually over the 1998–2008 sample period using accounting data from the prior five years. Thus, our earnings quality variables vary over time and across countries.

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[13] All offer sizes are measured in millions of CPI-adjusted 2008 U.S. dollars. CPI data are from the U.S. Bureau of Labor Statistics website. SDC reports offer size in U.S. dollars.

[14] In Japan all IPOs are priced on an integer. For Japanese IPOs, our integer indicator equals 1 if the offer price (in yen) is perfectly divisible by 100.

Our first set of earnings quality measures come from Leuz et al. (2003). They construct four country-level earnings management measures. The first of these, denoted EM1, is an earnings smoothing measure equal to the median ratio in country i of the firm-level standard deviations of operating earnings over the cash flow from operations (both scaled by lagged total assets). Their second measure, EM2, also attempts to capture earnings smoothing behavior. EM2 equals the cross-sectional correlation in country i in year t between the change in accruals and the change in cash flows (both scaled by lagged total assets). We transform our EM1 and EM2 measures by multiplying their values by -1, so that for EM1, EM2, and all other earnings quality measures, higher values correspond to more aggressive earnings management. Thus, our earnings quality hypothesis predicts a positive coefficient on the earnings quality measures in underpricing regressions, meaning that more earnings management leads to greater information asymmetry and higher underpricing.

The third measure, EM3, is the median value in each country of the absolute value of firms' accruals scaled by the absolute value of cash flow from operations. A higher value of EM3 indicates greater earnings management. The final metric, EM4, measures loss avoidance behavior. EM4 equals the ratio of the number of firms reporting small profits over the sum of the number of firms reporting small losses and small profits. In this context, "small" means a ratio of net income to assets of plus or minus 1 percent. The intuition for this measure is that if managers manipulate earnings to avoid showing losses, then there will be a missing probability mass in the earnings distribution just to the left of breakeven, and a higher-than-expected frequency of firms reporting earnings just above zero (see Degeorge et al. 1999). Therefore, the higher is the loss avoidance ratio, the greater is the incidence of loss avoidance behavior in a given country and the more opaque are the country's earnings figures. Finally, Leuz et al. (2003) calculate an overall earnings management figure, aggregate EM, for each country by ranking each country on each of the four earnings management measures and then taking the average ranking, where higher rankings signify more earnings management.

In addition to calculating two earnings management measures that replicate measures from Leuz et al. (2003), Bhattacharya et al. (2003) construct two additional earnings quality measures that focus on aspects of earnings opacity. The first unique measure is their earnings aggressiveness measure, defined as the tendency to accelerate the recognition of gains and delay the recognition of losses. Earnings aggressiveness for country i in year t is equal to the median value of the ratio of total accruals divided by lagged assets. A higher value of this ratio implies more aggressive (and more opaque) earnings. The second unique measure is the Bhattacharya et al. (2003) earnings opacity measure. They construct a single aggregate earnings opacity measure for each country based on a ranking methodology. Specifically, deciles are constructed for each of three earnings quality measures and then the decile ranks are averaged across the three measures to arrive at an overall earnings opacity ranking for each country. Countries that earn a higher average ranking have more opaque earnings. In the regression analyses that follow, we estimate the relation between each earnings quality measure and IPO underpricing.

Table 4 reports regressions that examine the relation between earnings quality and underpricing on a country-year basis. The dependent variable in the Table 4 regressions is the average IPO initial return in a given country and year. The control variables are country-year averages across all IPOs. There are 320 unique country-year combinations in our sample. The primary variables of interest in Table 4 are our aggregate EM measure in Model 1 and earnings opacity score in Model 2, where higher values are indicative of more earnings management. Our earnings quality hypothesis predicts a positive relation between our earnings management measures and IPO initial returns.

The Table 4 results support the prediction of our earnings quality hypothesis. Average underpricing is positively related to both aggregate EM and earnings opacity. The aggregate EM coefficient in Model 1 (0.004) implies that a one standard deviation improvement in a country's average aggregate EM measure is related to a reduction in average underpricing of 4.45 percentage points. The control variables in Table 4 are broadly consistent with expectations. The positive coefficients for IPO activity and market return are consistent with the "hot markets" effect. We also find that underpricing is positively correlated with the level of protection afforded to minority shareholders (as captured by the antidirector rights index) and stock return volatility. Underpricing is negatively correlated with the index of economic freedom.

TABLE 4

Country-Year Underpricing Regressions on Earnings Quality

	Model 1	Model 2
Intercept	0.392	0.105
Aggregate EM	0.004**	
Earnings opacity		0.063 ***
Top-tier underwriter	0.060	0.064
Price stabilization	−0.219	−0.396
IPO activity	1.839***	1.675 ***
Market return	0.740***	0.757 ***
Stock market turnover ratio	0.026	0.022
Index of economic freedom	−0.010***	−0.008 ***
Antidirector rights index	0.041***	0.026 *
Offer size ratio	0.000	−0.001
Stock return volatility	3.558***	3.777 ***
Integer offer price	−0.013	0.012
Bookbuilt	−0.093*	−0.077
Firm commitment	0.044	0.005
Equity carve-out	−0.016	−0.035
Adjusted R^2	27.64%	29.90%
Number of observations	320	320

*, **, *** Denote significance of the coefficient at the 10, 5, and 1 percent levels, respectively.

This table presents OLS regressions of IPO underpricing on country-level earnings quality measures. The dependent variable is the average IPO initial return measured at the country-year level. Initial return is calculated by dividing the secondary market closing price by the final offer price minus 1. The dependent variables are averaged at the country-year level and are defined in the notes to Table 3. Regressions control for industry composition. Industry classifications reflect those reported by Dyck and Zingales (2004).

To further examine the association between country-level earnings quality and IPO underpricing, in Table 5 we run pooled cross-sectional regressions of firm-level IPO underpricing on country-level measures of earnings quality and a variety of country- and firm-level controls.[15] In Models 1–4, we estimate regressions with each Leuz et al. (2003) earnings measure separately. In Model 5, we consider the aggregate earnings management measure. To the extent that earnings quality is related to information asymmetry, we expect poorer earnings quality to be associated with higher underpricing, on average.

Recall that for EM1, EM2, EM3, EM4, and the overall aggregate EM variable, a higher value means more earnings management. Therefore, our earnings quality hypothesis predicts positive coefficients for all these measures of earnings quality. The results from Models 1–4 provide support for the earnings quality hypothesis. Each of the earnings management measures enters the regression with a positive coefficient, as expected, although only two of the four coefficients are significant. When it is common practice in a country for managers to engage in activities to manipulate earnings, IPO underpricing is higher. Given the tendency of the individual earnings management variables to link to underpricing as expected, it is not surprising that, in Model 5, the aggregate EM score also indicates that poor earnings quality is associated with higher IPO underpricing.

In Table 5, Models 6 and 7, we examine the influence of the Bhattacharya et al. (2003) earnings aggressiveness and earnings opacity measures on IPO underpricing. These measures are constructed such that a higher value implies more opaque earnings. Therefore, we expect positive signs in the regressions. Indeed, both coefficients are positive and significant, consistent with the hypothesis that underpricing is higher in countries where investors receive lower quality accounting information.[16]

Although many of the control variables in Table 5 do not show a statistically significant link to underpricing, the variables that are significant generally have the expected sign. For example, IPOs taken public after a period of high market returns or those that display greater price volatility after trading begins have higher underpricing. Consistent with Boulton et al. (2010), initial returns tend to be greater for IPOs issued in countries offering stronger protections for minority shareholders, as indicated by the positive coefficient on the antidirector rights index. As expected, equity carve-outs are underpriced less than other IPOs, on average, as are firms going public in countries with greater economic freedoms. The adjusted R^2 values indicate that our models explain between 18 and 23 percent of the variation in the international underpricing cross-section.

Economically, the earnings quality effects presented in Table 5 are quite dramatic. The results in Model 5 suggest that, for a one standard deviation increase in a firm's country-level aggregate EM measure, underpricing increases by roughly 7.8 percentage points. This increase is more than one-fifth of the sample mean initial return of 36.5 percent. As a point of reference, consider that the aggregate EM measure for India is slightly more than one standard deviation above that of the United Kingdom. Clearly, country-level earnings quality has a considerable influence on underpricing around the world.

Robustness of Country-Level Earnings Quality and IPO Underpricing

In unreported regressions, we confirm that the results presented in Tables 4 and 5 are robust to a variety of alternative specifications. In particular, we attempt to eliminate concerns regarding

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[15] Because underpricing is likely to be correlated across IPOs within a country and across time, we construct two-way cluster-robust standard errors as outlined in Gow et al. (2010) for all firm-level IPO underpricing regressions.

[16] If we estimate the relation between underpricing and earnings quality in the presence of country-level fixed effects, the coefficients on the earnings quality measures are positive and significant about half the time.

TABLE 5

Underpricing Regressions on Earnings Quality Measures

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Intercept	1.606**	1.293*	1.282*	−0.684	1.171*	1.526**	0.699
EM1	0.455***						
EM2		0.183					
EM3			0.304				
EM4				1.796***			
Aggregate EM					0.007**		
Earnings aggressiveness						5.002***	
Earnings opacity							0.080**
Top-tier underwriter	0.044	0.050	0.047	0.076	0.054	0.052	0.066
Price stabilization	2.237	1.862	2.019	1.928	2.467	1.484	2.573
IPO activity	0.321	−0.019	0.124	0.432	0.183	0.057	0.139
Market return	0.892***	0.881***	0.875***	0.866***	0.876***	0.906***	0.868***
Stock market turnover ratio	−0.041	−0.043	−0.037	−0.004	−0.028	−0.036	−0.014
Index of economic freedom	−0.020**	−0.020**	−0.021**	−0.011**	−0.020**	−0.018**	−0.017**
Antidirector rights index	0.079**	0.062*	0.071*	0.086**	0.085*	0.040	0.069*
Offer size ratio	−0.001	−0.001	−0.001	−0.002	−0.001	−0.001	−0.001
Stock return volatility	3.949***	3.967***	4.048***	3.949***	4.027***	3.870**	4.002***
Integer offer price	−0.008	−0.004	−0.002	0.035	0.004	−0.011	0.019
Bookbuilt	−0.149	−0.136	−0.121	−0.126*	−0.135	−0.093	−0.109
Firm commitment	0.001	0.016	−0.004	0.011	−0.015	0.000	−0.049
Equity carve-out	−0.045	−0.059**	−0.052*	−0.056**	−0.051*	−0.054**	−0.058**
Adjusted R^2	18.43%	17.95%	18.05%	22.54%	18.60%	19.06%	19.57%
Number of observations	10,045	10,045	10,045	10,045	10,045	10,045	10,045

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*, **, *** Denote significance of the coefficient at the 10, 5, and 1 percent levels, respectively, measured using two-way cluster-robust standard errors (country and issue year) as outlined in Gow et al. (2010).

This table presents OLS regressions of IPO underpricing on country-level earnings quality measures. The dependent variable is the IPO initial return, which is the secondary market closing price divided by the final offer price minus 1. All other variables are defined in the notes to Table 3. Regressions include industry indicators. Industry classifications reflect those reported by Dyck and Zingales (2004).

the price-stabilization activity of underwriters, which can vary across countries, minimum offer price restrictions, which screen out the smallest IPOs, and the effects of large countries, which tend to be either very transparent or very opaque.

To confirm that our results are not sensitive to variations in price-stabilization activity across countries, we replace the one-day initial return measure used in Table 5 with the IPO initial return measured as the percentage difference between the offer price and the secondary market closing price 22 trading days after the first trading date. The intuition for this alternative measure is that the effects of price stabilization dissipate over time, generally within a month of the IPO, as reported in Ruud (1993) and elsewhere. Thus, if price stabilization temporarily obscures the left tail of the IPO returns distribution, then our finding that lower quality earnings leads to higher underpricing might be the result of greater price support in countries with lower earnings quality. We find that measuring the initial return over a longer horizon does not fundamentally change our results.

In other tests, we impose increasingly stringent restrictions on our country-level sample by imposing a minimum offer price cutoff. In particular, we exclude the bottom 2, 5, 10, and 20 percent of offer prices from respective country-level samples. In terms of the U.S. distribution of offer prices, these cutoffs correspond to minimums of $5, $6, $7, and $9, respectively. Increasing the minimum offer price reduces the odds of finding spurious results driven by market microstructure effects. In all cases, the results indicate that an economically and statistically significant increase in underpricing occurs when earnings quality deteriorates, and the magnitude of this effect is similar to our estimates in all prior regressions. We also exclude each of the three largest IPO markets in our sample, the U.S., the U.K., and Japan. We do this to verify that the earnings quality results are not driven exclusively by these large markets. Excluding any of these three countries, lower earnings quality is still associated with higher underpricing.

Earnings Quality and the Certification Effect of Financial Intermediaries

The previous section presented evidence that lower earnings quality is associated with higher underpricing. In the larger context of the IPO literature, we suggest that investors face greater uncertainty regarding the values of IPO firms when earnings quality is low. One mechanism that could mitigate this uncertainty is the presence of a reputable intermediary that, in effect, certifies the earnings of IPO firms. Carter and Manaster (1990), Megginson and Weiss (1991), and Barry et al. (1990) all report results consistent with a certification effect (i.e., lower underpricing) when firms go public with the assistance of reputable underwriters or venture capital investors.[17] However, recent underpricing studies, such as Loughran and Ritter (2004), find a positive relation between underwriter quality and initial returns. Why IPOs underwritten by more prestigious investment banks are underpriced more remains an unanswered question, but in spite of this general effect, it may be possible for high-quality underwriters to mitigate the effects of poor country-level earnings quality by providing a certification effect at the firm level.

Table 6 includes interactions between our top-tier underwriter variable and mean-adjusted earnings quality measures. In Model 1, the top-tier underwriter indicator variable is interacted with the mean-adjusted aggregate EM measure, while in Model 2 the interaction is with our earnings opacity measure. If top-tier underwriters certify the earnings of IPO firms and thereby reduce the uncertainty faced by investors, then we expect the interaction terms to have the opposite sign of the relevant earnings quality measure. In both regression specifications in Table 6, the coefficient on the interaction between top-tier underwriter and earnings quality is significant and takes the opposite sign of the earnings quality coefficient.

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[17] Due to missing data, reported results do not control for venture capital backing. Unreported tests indicate that all results are robust to controlling for venture capital backing where available.

TABLE 6
Underpricing Regressions on Earnings Quality with Underwriter Certification

	Model 1	Model 2
Intercept	1.133*	0.590
Aggregate EM	0.008***	
Earnings opacity		0.096***
Top-tier underwriter	0.042	0.048
Top-tier underwriter * earnings measure	−0.007**	−0.069*
Price stabilization	2.122	2.145
IPO activity	0.247	0.225
Market return	0.873***	0.870***
Stock market turnover ratio	−0.041	−0.027
Index of economic freedom	−0.020**	−0.016**
Antidirector rights index	0.080*	0.061
Offer size ratio	−0.001	−0.001
Stock return volatility	4.022***	4.023***
Integer offer price	0.006	0.022
Bookbuilt	−0.128	−0.098
Firm commitment	−0.009	−0.045
Equity carve-out	−0.047*	−0.054*
Adjusted R^2	18.86%	19.93%
Number of observations	10,045	10,045

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*, **, *** Denote significance of the coefficient at the 10, 5, and 1 percent levels, respectively, measured using two-way cluster-robust standard errors (country and issue year) as outlined in Gow et al. (2010).

This table presents OLS regressions of IPO underpricing on country-level earnings quality measures and the interaction of underwriter reputation and earnings quality. The dependent variable is the IPO initial return, which is the secondary market closing price divided by the final offer price minus 1. All other variables are defined in the notes to Table 3. Regressions include industry indicators. Industry classifications reflect those reported by Dyck and Zingales (2004).

The coefficients on the interaction terms indicate that the less transparent a country's earnings, the more IPO issuers benefit from underwriter quality. To illustrate the economic significance of this result, consider an issuer in Italy, the country with the median average earnings opacity score (average earnings opacity score = 5.49). The coefficient on the interaction term in Model 2 of Table 6 is −0.069 and indicates that an Italian IPO firm using a top-tier underwriter would underprice nearly 1.13 percentage points less than a similar Italian IPO firm not using a top-tier underwriter. Considering that the average IPO listed in Italy is underpriced by 10.46 percent, this result implies that the presence of a top-tier underwriter reduces underpricing by more than 10 percent for new issues in Italy. The effect of a top-tier underwriter is even more pronounced for issuers from countries with higher average earnings opacity scores. Table 6, Model 2 indicates that a Chinese IPO firm (average earnings opacity score = 8.03) would underprice nearly 18.66 percentage points less when using a top-tier underwriter as opposed to an underwriter not identified as top-tier. This represents a 15 percent decrease based on the average underpricing of Chinese IPOs (120.72 percent). We obtain similar results when examining the interaction between the other proxies for earnings quality and top-tier underwriters. These results are consistent with the presence of a top-tier underwriter acting as certification for new issues and are particularly important in countries with lower earnings quality.

V. CONCLUSION

Our primary contribution is to study how country-level differences in earnings quality influence IPO underpricing. Using a wide range of earnings quality measures, we find higher IPO underpricing in countries with poorer quality earnings information, even after controlling for many country- and deal-specific characteristics. This evidence is consistent with asymmetric information explanations for underpricing. Just as other researchers have found that poor accounting information can lead to a higher cost of capital, our evidence suggests that the cost of going public rises when investors have greater difficulties interpreting financial information. However, financial intermediaries, such as investment banks, play a role in mitigating the effects of low earnings quality on IPO underpricing.

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Are Investors Confused by Restatements after Sarbanes-Oxley?

Jeffrey J. Burks
University of Notre Dame

ABSTRACT: Regulators have expressed concern that investors are confused by the large number and questionable materiality of accounting restatements since passage of the Sarbanes-Oxley Act (SOX). This study looks for evidence of investor confusion by examining stock returns and trading volume. I find that the initial price reaction to restatement announcements becomes significantly less negative after SOX, even after controlling for the less egregious nature of post-SOX restatements. To assess whether the less negative reaction represents under-reaction, I test whether stock prices drift negatively over the months and years after the initial reaction. I find no evidence of statistically negative drifts unique to the post-SOX period. In fact, I find that post-SOX drifts are statistically less negative than pre-SOX drifts, suggesting that price efficiency actually improves after SOX. Finally, I find no evidence that post-SOX restatements have higher trading volume after controlling for contemporaneous returns, suggesting no increase in disagreement among investors about the restatements. Thus, the findings provide little evidence that investors are confused by post-SOX restatements.

Keywords: *accounting changes; accounting errors; restatements; Sarbanes-Oxley Act; regulation.*

Data Availability: *Data are available from public sources.*

I. INTRODUCTION

The sharp increase in the number of accounting restatements since passage of the Sarbanes-Oxley Act (SOX, U.S. House of Representatives 2002) has received considerable regulatory attention. In an op-ed piece for the *Financial Times*, United States Treasury Secretary Henry Paulson expressed concern that many of these restatements could needlessly alarm

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investors, leading to "confusion" over the quality of the financial reports.[1] Similarly, the Securities and Exchange Commission's Advisory Committee on Improvements to Financial Reporting (CIFR) fears that restatements "may create confusion that reduces the efficiency of investor analysis" (CIFR 2008, 78).

Regulators' concerns have a theoretical basis. Restatements likely increase processing costs for both institutional and individual investors, and several studies theorize that investors have difficulty impounding costly information into asset prices (Grossman and Stiglitz 1980; Bloomfield 2002; Hirshleifer and Teoh 2003). Due in part to these concerns, CIFR recommends reducing the scope of violations that necessitate a restatement. However, these recommendations are opposed by some investor groups that believe investors benefit from the restatements' transparent portrayal of prior period effects (Johnson 2008).

This study looks for signs of investor confusion in the price and volume reactions to post-SOX restatements, which help regulators assess whether investor confusion is a valid rationale for curtailing restatements. The pricing tests examine whether investors systematically over- or under-react to restatements. Although Paulson's comments suggest a fear that investor reaction to the increased number of restatements will be overly negative, investors could also respond by ignoring or under-reacting to restatements due to the additional processing costs. CIFR Chairman Robert C. Pozen expresses uncertainty over whether investors would over- or under-react: "A restatement imposes obvious costs on a preparer, but it also imposes two sorts of costs on investors. One is confusion: If a restatement used to mean to investors 'this is a really big problem,' and you get a lot of restatements that aren't big problems, then we haven't sorted that out" (Cummings 2008, 1).[2] Pozen also suggests that, with so many restatements to track, investors may fail to distinguish the egregious restatements from those that are more innocuous (Heffes 2007).[3] "Confused" investors who fail to discern differences in egregiousness across restatements would likely under-react to the more egregious restatements and over-react to the less egregious restatements.

I test for over- or under-reaction primarily by examining long-run stock price drifts following restatements. However, I first examine the initial stock price reactions to restatements, benchmarking the initial reactions in the post-SOX period against those in pre-SOX period, when restatements were less frequent and firms faced less pressure to restate for errors of questionable materiality. Prior studies note that price reactions to restatement announcements become less negative after SOX (U.S. Government Accountability Office [GAO] 2006; Hranaiova and Byers 2007; Scholz 2008), which is consistent with investors under-reacting in the post-SOX period. However, the less negative reactions may be warranted if post-SOX restatements are less egregious, as regulators claim. I find that, after controlling for the less egregiousness nature of post-SOX restatements, post-SOX reactions continue to be less negative.

If less negative initial reactions are truly under-reactions, then stock prices should continue to drift downward as prices converge to fundamental value. Contrary to claims of higher investor confusion after SOX, I find that post-SOX restatements have significantly less drift than do pre-SOX restatements. I also test for drifts in subsamples of restatements in case the full sample exhibits no drift simply because investors under-react to more egregious restatements and over-react to less egregious restatements. Consistent with the full sample result, the subsample analysis provides no evidence of drift unique to the post-SOX period.

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[1] Paulson stated, "Restatements pose significant costs on our capital markets. They have the potential to confuse investors and erode public confidence in financial reporting. Some of these restatements might not be material to investors, and others may simply reflect new accounting standards interpretations" (Paulson 2007, 15).

[2] The second cost to investors Pozen mentions is the delay in earnings announcements and other disclosures while firms undergo the process of restating financial statements. Badertscher and Burks (2010) examine this issue.

[3] Pozen stated, "I think the evidence is that many restatements seem to have very little market effect. On the other hand, there are some restatements with significant market impacts, so we should be helping preparers and investors sort out which ones are which" (Heffes 2007, 17).

The pricing tests may not detect investor confusion because prices aggregate investor beliefs. Prices can be efficient even in a market containing confused investors if overly optimistic investors trade with overly pessimistic investors, or if the market includes rational arbitrageurs with enough power to correct the influence of the confused investors. In either case, the presence of confused investors would lead to trading volume that is independent of price movements (Kim and Verrecchia 1997). Thus, as a final test for investor confusion, I test for an increase in trading volume after controlling for price movements around post-SOX restatement announcements. I do not find a statistically discernable trading volume increase for post-SOX restatements. In summary, the results of the pricing and volume tests do not support the notion that the increase in restatements after SOX produced investor confusion.

This study contributes to the debate between regulators and investor groups over proposed reforms that would curtail the use of restatements for accounting errors. Regulators' concerns about investor confusion have fostered momentum for the reforms. However, this study's finding that investors price restatements more efficiently after SOX than before suggests that investor confusion is not a valid rationale for curtailing restatements.

The study also informs a broader standard-setting question about investors' capacity to process restatements for accounting changes other than error corrections. Fears about investor confusion have caused U.S. standard-setters to prohibit restatements for many types of accounting changes since 1971 (Accounting Principles Board [APB] Opinion No. 20, APB 1971, para. 14b). In 2005 the Financial Accounting Standards Board partially reversed course and began requiring firms to restate for changes in accounting principle (Statement of Financial Accounting Standards [SFAS] No. 154, FASB 2005). Some academics have advocated further expanding the use of restatements to changes in accounting estimates (Lundholm 1999; Lev 2003). My study suggests that investors were able to adapt to a dramatic increase in restatements for accounting errors, leaving open the possibility that investors could adapt to an environment with even more types of restatements.

II. BACKGROUND

Controversy Over the Increase in Restatements after SOX

Studies by the GAO of the period 1997 to 2005 show a marked increase in restatements beginning in 2002, the year SOX was passed (GAO-06-678 [GAO 2006], GAO-06-1053R [GAO 2007]). This increase is depicted in Figure 1 and is discussed further in Section IV.[4] Since the increase in restatements begins in 2002, I refer to it as a "post-SOX" phenomenon, although SOX may not be the sole cause. Both SOX and the related high-profile accounting scandals (e.g., Enron and WorldCom) likely prompted managers and auditors to become more vigilant in detecting and correcting accounting errors, leading to more restatements.

Regulators have several concerns about the increase in restatements, including the potential for investor confusion, the burden on preparers, and reporting delays involved with the restatement process (CIFR 2008). This study complements other academic work addressing these concerns (Wright et al. 2008; Badertscher and Burks 2010) by testing for evidence of investor confusion, a particular point of contention between regulators and investor groups.

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[4] The GAO sample ends in 2005. Tracking by research firm Glass, Lewis & Co., LLC and by Audit Analytics, a research arm of the Ives Group, Inc., shows that restatements peaked in 2006, but remained high in 2007 and 2008, dwarfing the restatement counts of years in the late 1990s by factors of 5 to 10 (Taub 2009).

FIGURE 1
Restatement Counts by Year

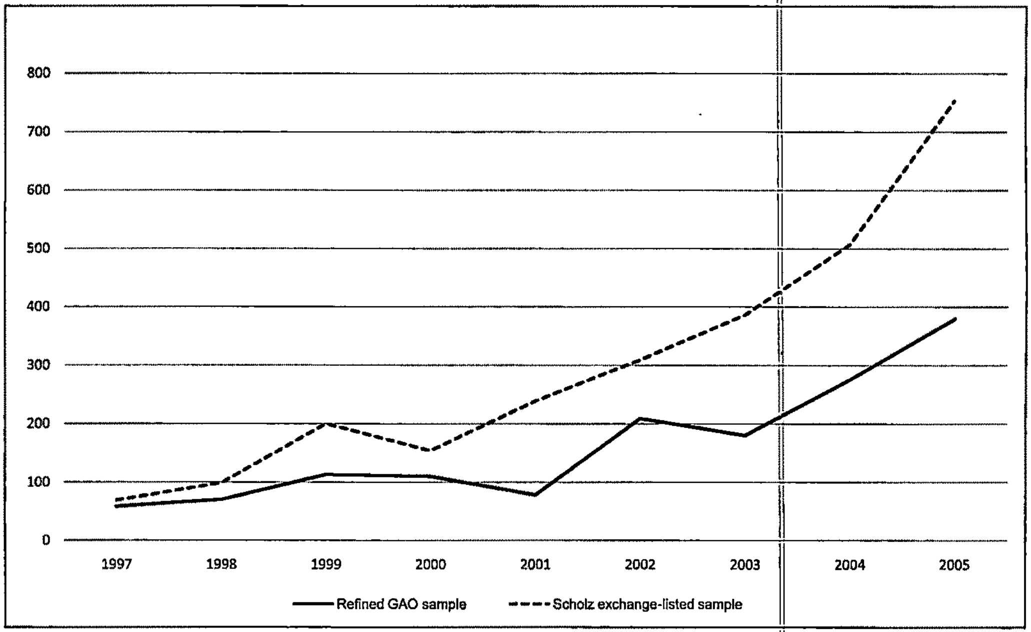

Figure 1 shows sample size by year for the GAO sample after the refinements described in Table 1 are applied. For comparison, sample counts for exchange-listed companies from Scholz (2008), Figure 1 are shown.

Policymakers' opposition to revisions of prior-period financial statements dates back to APB Opinion No. 20 in 1971 (Lev et al. 2008), and stems from concern about how investors perceive and process restatements: "Restating financial statements of prior periods may dilute public confidence in financial statements and may confuse those who use them" (APB Opinion No. 20, para. 14b). Thus, the APB prohibited restatements for changes in accounting principle and estimates, but did require restatements for accounting errors.

As the number of restatements ballooned after 2002, concerns about investor confusion resurfaced and played a role in controversial proposals to reform restatement practices. After Secretary Paulson's op-ed piece in the *Financial Times*, the Treasury Department commissioned a study examining the increase in restatements and capital market effects (Scholz 2008). The SEC formed the CIFR, which recommended ways to reduce restatements, proposing that more errors be classified as immaterial so restatements would not be necessary (SFAS No. 154). Under the CIFR's proposals, even if the cumulative impact of an error is large, the error would be classified as immaterial if it relates to business activities that do not drive firm value or risk, or if the error is immaterial to each prior period viewed in isolation (CIFR 2008, 81–83). Some observers, including a former SEC Chief Accountant, claim that the SEC has informally adopted these or other restatement-reducing policies (Rummell 2008; Stuart 2010).

Many investor groups oppose curtailing restatements, claiming that investors are capable of assessing their value relevance. At a public forum hosted by CIFR, an analyst for the Capital

Group Cos. stated that "investors want to be their own decision-makers of which errors are unimportant in their investment theses" (Johnson 2008, 1). In a comment letter to the CIFR, the Consumer Federation of America (CFA) stated that attempting to eliminate so-called unnecessary restatements "poses risks to investors that far exceed any potential benefits" (CFA 2008, 3). The Chartered Financial Analyst Institute (CFA Institute) state that "it is better to err on the side of providing too much disclosure rather than too little. We note that investors use a wide variety of investment approaches and models, so attempting to determine if certain changes might alter certain approaches could be problematic" (CFA Institute 2008, 19).

Despite these assertions, regulators' concerns about investors' ability to value restatements may have merit. To efficiently price restatements, investors must convert the revised financial statements to usable form. D'Souza et al. (2010) emphasize that many institutional investors rely on data aggregators like Standard & Poor's (S&P) to organize financial information into a standardized machine-readable form. However, as explained to me by an S&P manager, S&P's collection procedures prioritize current filings ahead of amended filings. Amended filings made during peak times of the year often take forty to eighty days to reach clients.

Consequently, investors who want immediate access to restated financial statements would need to set up their own processes for monitoring amended filings, inputting data, and controlling quality control, endeavors that become more costly as the number of restatements increases. The S&P manager reports that monitoring amended filings is difficult because they are not scheduled like earnings announcements, and are made for a host of reasons other than accounting restatements, such as missing signatures or unsatisfactory qualitative disclosures.[5] The restatement footnote within the amended filing often does not provide restated versus original comparisons for all periods affected. To identify changes in longer historical trends, the investor must check each figure in the five-year summary table of "selected financial data" (item 6 of the 10-K) to determine whether it has been restated, a process the S&P manager describes as "cumbersome." If a data aggregator like S&P finds these steps difficult, then many institutional investors likely find them difficult as well.

Mispricing could result even if arbitrageurs are willing to bear the collection costs. Black (1986) and Shleifer and Vishny (1997) discuss general limits to risky arbitrage, and several studies specifically focus on the limits related to costly information. Hirshleifer and Teoh (2003) examine a setting in which investors must process vast amounts of available information, and show that investors with "limited attention" create mispricing that fully attentive investors do not arbitrage away because of risk aversion. Grossman and Stiglitz (1980) argue that there is an equilibrium degree of mispricing in markets because arbitrageurs require trading gains as compensation for costly information searches. They show that the degree of mispricing increases in the cost of information. Bloomfield (2002) also bases his "incomplete revelation hypothesis" on the notion that arbitrageurs must be compensated for costly information search; the hypothesis holds that investors can misprice even widely publicized events like restatements because of "the costs of identifying, collecting, compiling, printing and processing data, or hiring others to do so" (Bloomfield 2002, 236). In light of the apparent increase in information costs that accompanies the large number of post-SOX restatements, my study tests whether price efficiency declines after SOX.

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[5] In August 2004, the SEC made accounting restatements easier to identify by requiring announcement in a Form 8-K when management concludes that prior financial statements should not be relied upon. However, firms still do not file 8-Ks for nearly one-third of restatements even after passage of the rule (Myers et al. 2010). Even when an 8-K is filed, the firm may not file amended financial statements until several weeks or months later, requiring investors to keep watch.

Prior Studies on Market Reaction to Restatements

One stream of prior research shows that the average market reaction to restatement announcements becomes less negative after SOX (GAO 2006; Hranaiova and Byers 2007; Scholz 2008). A second stream of research finds that restatements become, on average, less egregious after SOX; post-SOX restatements commonly involve lower dollar amounts, unintentional errors, and non-core accounts (Burks 2010; Hennes et al. 2008; Plumlee and Yohn 2010; Scholz 2008). Several studies document that these restatement characteristics affect market reaction (e.g., Palmrose et al. 2004a; Gordon et al. 2008; Hennes et al. 2008; Files et al. 2009). Thus, I merge the two streams of literature to determine whether the less egregious nature of post-SOX restatements can completely explain the less negative market reactions. Two concurrent studies also examine reasons for the decline in market reaction (Hirschey and Smith 2009; Huang 2010). Like my study, both find that changes in restatement characteristics do not fully explain the less negative post-SOX reactions. A unique contribution of my study is that it tests whether mispricing, a chief concern of regulators, is the reason that changes in restatement characteristics fail to explain the decline in market reaction.

Another unique feature of my study is that it tracks the date when firms disclose the restatement's earnings impact. Firms often do not disclose an error's full impact at the initial announcement of a pending restatement; the process of quantifying the errors can take several weeks or months. Prior studies focus on the price reaction during a relatively short window around the initial announcement when investors often do not have complete information about the restatement. I examine the price reaction using a short event window as in prior studies, but also use an "episode" window that spans from the initial restatement announcement to the release date of the earnings impact. I also incorporate the release date of the earnings impact into the long-run price drift tests, to distinguish drifts due to slow release of information from drifts due to investors mispricing released information.

III. RESEARCH DESIGN

Announcement Returns

I use the following model to assess whether restatement market reactions become less negative after SOX simply because restatements become less egregious:

$$Return_i = \alpha + \beta_1 POSTSOX_i + \beta_2 RSTMT\ CONTROLS_i + \beta_3 GENERAL\ CONTROLS_i + \varepsilon_i. \quad (1)$$

$Return_i$ is the size-adjusted, buy-and-hold stock return of firm i measured over one of two windows: the three-day window $(-1, +1)$ around the initial restatement announcement (*RETURN_ANNOUNCE*), or a window beginning one day before the initial restatement announcement and ending one day after the earnings impact announcement (*RETURN_EPISODE*). The earnings impact announcement is when a firm reveals the restatement's impact on past earnings, either cumulatively or by period. The announcement must be definitive, not an estimate, and can come in a press release before the firm actually files amended financial statements with the SEC.

The variable of interest is *POSTSOX*, an indicator that captures whether the restatement is announced in the month SOX was passed (July 2002) or after. Although prior studies document less negative announcement returns to post-SOX restatements, I do not predict the *POSTSOX* coefficient sign because announcement returns could be higher, lower, or the same after controlling for the post-SOX decline in egregiousness.

I briefly describe two sets of control variables in this section; the Appendix includes detailed descriptions of the variables. The first set of controls relates to restatement characteristics. I include the magnitude of the restatement's earnings impact scaled by total assets (*MAG*), an indicator variable for nonnegative earnings impacts (*POSITIVE_MAG*), and an interaction between them. *MAG* and *POSITIVE_MAG* are expected to be positively related to announcement returns. I expect a negative sign on the interaction term's coefficient because earnings-increasing restatements are not necessarily good news and so may not garner positive returns, thus attenuating *MAG*'s effect. I also include a term that multiplies *MAG* by its absolute value ($MAG * |MAG|$) in case investors do not react as strongly per dollar to restatements with extreme earnings impacts (Lipe et al. 1998).

I expect the following restatement characteristics to positively affect returns: whether the restatement is prompted by management as opposed to the auditor or SEC (*MGT_PROMPTS*) and whether the restatement involves quarters of the current fiscal year only (*QUARTERLY_ONLY*). I expect the following restatement characteristics to negatively affect returns: whether the definitive earnings impact is unknown at the time of the initial announcement (*NO_INITIAL_MAG*); whether the firm takes more than 30 days to disclose the earnings impact after the initial announcement (*LONG_MAG_DELAY*); whether the restatement results from fraudulent actions at the corporate level (*FRAUD*) or lower (*FRAUD_LOWLEVEL*); whether the restatement affects components of pre-tax operating income other than non-cash items like depreciation (*CORE*); and whether the restatement involves multiple errors (*MULTIPLE*). To form *MULTIPLE*, I place each restatement into one of 11 mutually exclusive categories based on the items restated. *MULTIPLE* equals 1 when the restatement spans categories or involves three or more errors in the same category.

The second set of controls relates to general factors that potentially affect the market reaction to restatements. To control for pre-announcement information leakage or industry-spillover effects (Gleason et al. 2008), I use size-adjusted stock returns in the 180 days preceding the announcement (*RETURN_PRIOR*), expecting a negative coefficient. I use the natural log of market capitalization (*FIRMSIZE*) to control for reputational effects that could mitigate negative reactions to restatements (Gordon et al. 2008). Following Scholz (2008), I include the mean value of the Chicago Board Options Exchange's volatility index over the return window (*VOLATILITY_INDEX*) because reactions to bad news may be more pronounced when market volatility is high. Finally, I control for surprises associated with any earnings announcements in the return window (*ESURPRISE* or *ESURPRISE_EPISODE*), and whether the earnings announcements meet or beat the consensus estimate (*MBE*). *MBE* is not included in the episode return regression because the episode often contains more than one earnings announcement.

Long-Run Price Drifts

I test the efficiency of investor reaction to restatements by investigating post-announcement price drift.[6] I measure drifts using Lyon et al.'s (1999) method for computing abnormal buy-and-hold returns. First, I restrict the analysis to ordinary common shares (CRSP share codes 10 and 11). Then I form benchmark portfolios by partitioning all CRSP/Compustat NYSE firms into size-based deciles using market value of common equity at the beginning of the calendar year.[7] I then add Nasdaq and AMEX firms to the deciles (using the NYSE breakpoints), and partition the lowest decile into quintiles, which creates a total of 14 size portfolios. I then divide each size portfolio into book-to-market quintiles, creating 70 benchmark portfolios.

To compute the return for a benchmark portfolio, I compute the buy-and-hold return over the horizon for each firm in the portfolio and then average across firms. If the firm stops trading before the horizon ends, then I incorporate the delisting return as in Beaver et al. (2007) and use the portfolio-level return for the remaining days.[8] I do not rebalance or allow new firms to enter the benchmark portfolios within the horizon because this induces bias (Lyon et al. 1999). I then subtract the benchmark portfolio's buy-and-hold return from the restatement firm's buy-and-hold return to compute the abnormal buy-and-hold return.

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[6] Price drifts could result from compensation for unidentified risk factors rather than mispricing. However, to the extent any unidentified risk factor operates similarly in the pre- and post-SOX periods, such a factor could not explain the *differences* in drift (the focus of this study).

[7] Lyon et al. (1999) compute market value of equity in July of each year. I use the beginning of the year instead because many restatements are announced in the first quarter, so using July may create a look-ahead bias.

Lyon et al. (1999) find that skewness in buy-and-hold returns affects statistics that rely on normality, and recommend evaluating significance using an empirical distribution of 1,000 "pseudo-portfolios." Each pseudo-portfolio consists of a set of control firms that matches the size and book-to-market profile of the set of restatement firms. To form a single pseudo-portfolio of control firms, I match each restatement firm randomly with replacement to a non-restatement firm in the same size/book-to-market benchmark portfolio. Then I compute an abnormal return for each control firm in the pseudo-portfolio as described above, and find the median abnormal return of the pseudo-portfolio. This process is repeated 1,000 times to form an empirical distribution of median abnormal returns. I then assess the statistical significance of the restatement firms' median abnormal return using its ranking in the empirical distribution.[9] I focus on medians to reduce the influence of outliers; the study seeks to test whether investors typically under-react or over-react to restatements, not whether a trading strategy to exploit the mispricing is executable.

Abnormal Volume

Even if prices are efficient, confusion could still exist among investors because prices reflect aggregate, not individual, beliefs. In the aggregate, the beliefs of rational investors may dominate those of confused investors, or the overly positive beliefs of some confused investors may offset the overly negative beliefs of other confused investors. Kim and Verrecchia (1997) model how these types of disagreements across investors cause higher levels of trading volume for a given level of contemporaneous returns. The idea is that disagreements over underlying firm value generate trading, but the lack of consensus hinders movements in price (Beaver 1968).[10] If confusion over post-SOX restatements reduces consensus across investors, then post-SOX restatements should have higher trading volume after controlling for contemporaneous returns. I test for this effect using the following model:

$$VOLUME_i = \alpha + \beta_1 POSTSOX_i + \beta_2 RSTMT\ CONTROLS_i + \beta_3 GENERAL\ CONTROLS_i + \varepsilon_i. \tag{2}$$

To adjust the dependent variable for market-wide volume and the firm's normal volume, I follow the method of Garfinkel and Sokobin (2006): first scale the firm's daily volume by shares outstanding to compute "turnover," or the percentage of shares outstanding traded that day; compute a market-wide turnover measure using NYSE and AMEX common stocks, and subtract this from the firm's turnover; and then compute average market-adjusted turnover (TO) over the three-day window around the restatement announcement:

$$TO_{restate,i} = \left\{ \sum_{t=-1}^{1} \left[\left(\frac{Vol_{i,t}}{Shs_{i,t}}\right)_{firm} - \left(\frac{Vol_t}{Shs_t}\right)_{mkt} \right] \right\} \Big/ 3 \tag{3}$$

where $Vol_{i,t}$ is firm i's volume on day t, and $Shs_{i,t}$ is firm i's shares outstanding on day t. Finally, to adjust for the firm's normal level of TO, compute the average TO over trading days (−250,

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[8] If the delisting return is missing, I use a replacement value based on the mean for that delisting code from Beaver et al. (2007, Table 3).

[9] Similar pseudo-portfolio methods are used by Ikenberry et al. (1995), Lee (1997), and Byun and Rozeff (2003).

[10] Other studies that infer disagreement from higher trading volume after controlling for returns include Bamber and Cheon (1995), Kandel and Pearson (1995), Bamber et al. (1997), and Bailey et al. (2003).

−31) relative to the restatement announcement (TO_{pre}), and subtract from the announcement window *TO*:

$$VOLUME_i = TO_{restate,i} - TO_{pre,i}. \tag{4}$$

The explanatory variable of interest in Equation (2) is *POSTSOX*; a positive coefficient sign is consistent with post-SOX restatements generating more disagreement among investors. The main control variable is the absolute value of the abnormal return in the three-day restatement announcement window (*AB_RETURN*). I interact *AB_RETURN* with an indicator variable for positive abnormal returns (*POSITIVE_RETURN*) to allow for different volume reactions to good and bad news (Karpoff 1987).[11] Following Bailey et al. (2003), I also interact *AB_RETURN* with *POSTSOX* in case the relation between volume and returns changes after SOX.

As additional controls for news, I include the restatement characteristics from the model of announcement returns, using absolute values for earnings magnitude (*AB_MAG*), earnings surprise (*AB_ESURPRISE*), and returns leading up to the announcement (*AB_RETURN_PRIOR*). *AB_RETURN_PRIOR* is expected to negatively affect *VOLUME* because large unsigned returns before the restatement suggest news leakage that reduces the news in the restatement announcement. Because the revelation of erroneous accounting is fundamentally bad news, I expect variables that worsen the news associated with a restatement to positively affect volume (*FRAUD*, *LONG_MAG_DELAY*, etc.), and variables that lessen the bad news to negatively affect volume (*POSITIVE_MAG*, *MGT_PROMPTS*, and *QUARTERLY_ONLY*). Finally, I expect the *VOLATILITY_INDEX* to be positively related to volume because restatement news could generate more disagreement among investors during times of high uncertainty.

IV. SAMPLE SELECTION

To form the sample, I use two reports from the GAO (GAO-03-138 [GAO 2002] and GAO-06-678 [GAO 2006]) that identify restatements from 1997 to September 2005. The two reports use consistent methods for identifying restatements across the pre- and post-SOX periods.[12] The reports focus on restatements of exchange-traded firms, which improves the chances that firm data are available on CRSP, Compustat, and I/B/E/S.

Before hand-collecting information about each restatement, I eliminate 323 of the 2,309 restatements because they lack basic data on CRSP and Compustat in the year of or year before the restatement. I eliminate another 513 restatements in the process of hand-collecting, leaving 1,473 restatements. The most common reasons for elimination are that the error relates to an earnings release for the current period rather than a prior period 10-K or 10-Q (114), or the GAO captures more than one announcement for the same restatement (107). Table 1 lists other reasons for elimination.

The GAO reports the announcement date of each restatement. However, I find earlier disclosures mentioning the possibility of restatement for 250 of the 1,473 restatements, with a mean (median) difference of 47 (22) days. I identify earlier disclosures because the GAO requires companies to imply that a restatement is at least "likely" (GAO-06-678, 56), whereas I look for the

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[11] Based on prior studies, Karpoff (1987) concludes that volume is higher around good news events than bad news events. I do not predict a sign for my *AB_RETURN* ∗ *POSITIVE_RETURN* interaction because the bad-news nature of restatements means that my sample is fundamentally different from studies Karpoff (1987) reviews, which examine the general relation between volume and price changes.

[12] Although the GAO issued the reports several years apart, in the second report the GAO states, "To determine the number of and reasons for restatements since 2002, we employed substantially the same methodology used in our prior report, in which we analyzed the period from January 1997 through June 2002" (GAO 2006, 52).

TABLE 1
Sample Selection

Restatements Identified by the GAO from 1997 to September 2005	2,309
1. Missing basic Compustat and CRSP data in year of or year before restatement	−323
2. Firm is amending an earnings release rather than a prior form 10-K or 10-Q	−114
3. Subsequent announcements related to the same restatement	−107
4. Adopting SAB No. 101	−72
5. Adopting a new standard	−65
6. Firm never files restated financials because of bankruptcy, acquisition, etc.	−33
7. Restatement impact is not released in US dollars	−25
8. Changing from one within-GAAP method to another	−24
9. Firm is not an SEC filer	−16
10. Firm decides not to restate after the initial announcement	−16
11. Other	−41
Base Sample	1,473

Observations vary by table depending on data availability. I exclude restatements due to adoption of SEC Staff Accounting Bulletin No. 101 (step 4 above) because of controversy over whether the bulletin represented new GAAP or was simply a reiteration of existing GAAP (Altamuro et al. 2005).

first mention of possible accounting errors. Using the earliest date mentioned allows me to more accurately capture the market reaction to the entire restatement episode, from the first mention of accounting problems to the revelation of the earnings impact.

Figure 1 plots the number of restatements by year in my sample and includes a comparison plot of the Scholz (2008) sample. Two differences are apparent. First, the GAO sample has fewer restatements each year. The GAO has fewer restatements from 1997 to 2000 likely because Scholz (2008) supplements the GAO list for these years with her own Lexis-Nexis searches (Scholz 2008, 8). Beginning in 2001, Scholz (2008) switches to the Audit Analytics database, which uses procedures that are more comprehensive than the GAO's to identify restatements. The large difference in 2005 likely arises because the GAO ends its search in September of that year but Audit Analytics covers the entire year. The observations I eliminated in Table 1 may also account for some of the differences.

The second major difference between the two samples is that the increase in restatement activity begins in 2002 for the GAO sample and 2001 for the Scholz (2008) sample. Scholz (2008) reports that the increase in her sample may be caused by the switch to Audit Analytics in 2001, as Audit Analytics uses more thorough search technologies and often includes multiple announcements for the same restatement (Scholz 2008, 8, 11).[13]

V. RESULTS

Descriptive Statistics

Data requirements for the regressions further reduce the sample of restatements from 1,473 to 1,226.[14] The pre-SOX (post-SOX) period contains 407 (819) restatements. Table 2 presents pre- and post-SOX comparisons of the variables. To reduce the influence of outliers, I winsorize the

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[13] Scholz (2008) attempts to remove duplicate announcements by eliminating subsequent observations for the same firm over the next 90 days.

[14] Missing I/B/E/S information causes most of the sample reduction. Regression results are similar when using the larger sample without the I/B/E/S-related controls.

TABLE 2
Descriptive Statistics

	Pre-SOX (n = 407)			Post-SOX (n = 819)			Difference	
	Mean	Median	Std. Error	Mean	Median	Std. Error	Mean	Median
Dependent Variables								
RETURN_ANNOUNCE	−0.102**	−0.060**	0.167	−0.033**	−0.015**	0.106	0.069**	0.046**
RETURN_EPISODE	−0.147**	−0.071**	0.249	−0.043**	−0.015**	0.171	0.103**	0.056**
VOLUME	0.018**	0.002**	0.052	0.006**	0.001**	0.022	−0.012**	−0.001*
Variable of Interest								
POSTSOX	0.000	0.000	0.000	1.000	1.000	0.000	NA	NA
Control Variables								
Restatement Characteristics								
MAG	−0.041**	−0.011	0.109	−0.023**	−0.004	0.070	0.018**	0.007**
POSITIVE_MAG	0.214	0.000	0.410	0.275	0.000	0.447	0.061*	0.000
NO_INITIAL_MAG	0.568	1.000	0.496	0.637	1.000	0.481	0.070*	0.000
LONG_MAG_DELAY	0.359	0.000	0.480	0.385	0.000	0.487	0.026	0.000
FRAUD	0.349	0.000	0.477	0.214	0.000	0.410	−0.135**	0.000
FRAUD_LOWLEVEL	0.076	0.000	0.266	0.040	0.000	0.197	−0.036**	0.000
MGT_PROMPTS	0.472	0.000	0.500	0.714	1.000	0.452	0.243**	1.000
CORE	0.398	0.000	0.490	0.233	0.000	0.423	−0.165**	0.000
MULTIPLE	0.221	0.000	0.416	0.248	0.000	0.432	0.027	0.000
QUARTERLY_ONLY	0.400	0.000	0.491	0.168	0.000	0.375	−0.232**	0.000
POSTIVE_RETURN	0.268	0.000	0.443	0.353	0.000	0.478	0.085**	0.000
General Controls								
RETURN _PRIOR	−0.184**	−0.239**	0.424	−0.021	−0.023**	0.330	0.163**	0.216**
FIRMSIZE	12.262	12.054	2.081	12.960	13.003	1.870	0.699**	0.949**
VOLATILITY_INDEX	23.982	23.200	4.015	18.336	15.160	7.688	−5.646**	−8.040**
ESURPRISE	−0.002**	0.000	0.013	−0.001**	0.000	0.010	0.001	0.000
ESURPRISE_EPISODE	−0.019**	0.000	0.070	−0.009**	0.000	0.048	0.011*	0.000
MBE	0.128	0.000	0.334	0.188	0.000	0.391	0.060**	0.000

(continued on next page)

TABLE 2 (continued)

*, ** Denote the value is significantly different from 0 at the 5 percent and 1 percent levels, respectively (two-tailed).

t-tests (Wilcoxon signed-rank tests) are used for assessing means (medians). For continuous variables, difference-in-means t-statistics are computed assuming unequal variances, and differences in medians are assessed using Wilcoxon two-sample tests. For dichotomous variables, differences are assessed using Chi-squared tests. Statistical significance is reported only when the null hypothesis is 0. *ESURPRISE_EPISODE* has only 352 pre-SOX and 716 post-SOX observations due to missing data on I/B/E/S.

See the Appendix for descriptions of variables.

following variables at the 1st and 99th percentiles: *RETURN_ANNOUNCE*, *RETURN_EPISODE*, *MAG*, *RETURN_PRIOR*, *ESURPRISE*, and *ESURPRISE_EPISODE*.

Mean restatement announcement returns (*RETURN_ANNOUNCE*) are significantly negative in both periods, but are much less negative in the post-SOX period (−10.2 percent pre-SOX versus −3.3 percent post-SOX), consistent with prior research (GAO 2006; Scholz 2008). Mean returns measured over the entire episode (*RETURN_EPISODE*) are even more negative, and the difference in returns between the two periods is even larger (−14.7 percent pre-SOX versus −4.3 percent post-SOX). The same trends hold for median returns. To understand when the decline in market reaction begins, Figure 2 plots median *RETURN_EPISODE* by year. The decline in reaction roughly coincides with the increase in the number of restatements that began in 2002; median returns for each year from 2002 to 2005 are noticeably less negative than those from 1997 to 2001 (depicted with gray bars in the figure). Among just the non-fraudulent restatements, the drop in

FIGURE 2
Median Episode Returns by Year (*RETURN_EPISODE*)

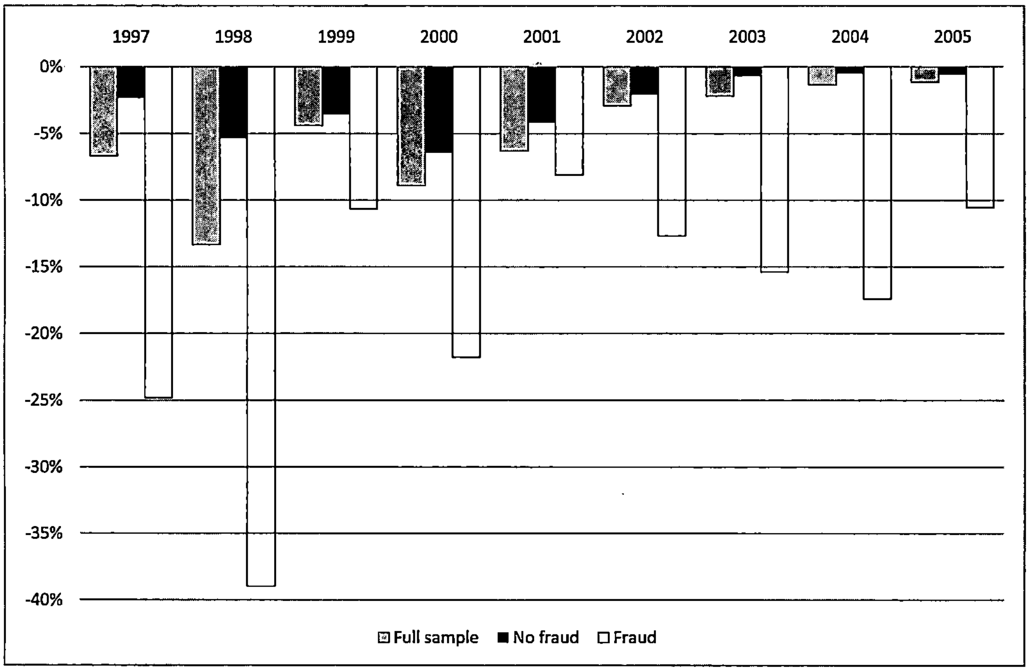

Figure 2 shows the median size-adjusted returns of restatement firms beginning one day before the restatement announcement and ending one day after the earnings impact announcement.

median returns occurs in 2002 or 2003 (depicted with black bars). No trend is evident in the returns of fraudulent restatements (depicted with white bars).[15]

Mean and median abnormal *VOLUME* in the three-day window around the restatement announcement is significantly positive in both periods. *VOLUME* is significantly lower after SOX, even though investor disagreement would tend to generate more volume. Part of the reason for the lower post-SOX *VOLUME* is that post-SOX restatements are less newsworthy, as evidenced by the less negative announcement returns. Later I use regression (2) to determine whether *VOLUME* is higher after SOX, holding newsworthiness constant.

Consistent with the less negative returns after SOX, post-SOX restatements are less egregious across many dimensions. Post-SOX restatements' mean impact on earnings as a percentage of assets (*MAG*) is only about half that of pre-SOX restatements (−2.3 percent post-SOX versus −4.1 percent pre-SOX). Post-SOX restatements are less likely to involve *FRAUD* (21.4 percent post-SOX versus 34.9 percent pre-SOX), more likely to be prompted by management (*MGT_PROMPTS*, 71.4 percent post-SOX versus 47.2 percent pre-SOX), and less likely to involve *CORE* earnings items (23.3 percent post-SOX versus 39.8 percent pre-SOX).[16]

Announcement Return Results

Table 3 presents coefficient estimates from the announcement return regression (1) using OLS with heteroscedasticity-consistent standard errors (White 1980). One specification uses short window returns as the dependent variable and the other uses episode returns. The episode return regression has fewer observations because it excludes observations that are missing I/B/E/S data for any earnings announcement in the episode window. In both regressions, the coefficient on *POSTSOX* is significantly positive, statistically and economically, suggesting that investors react less negatively to restatements after SOX even after controlling for the decline in egregiousness. In the short window (episode window) regression, the *POSTSOX* coefficient is 0.0464 (0.0597), suggesting that mean returns are 4.64 (5.97) percentage points higher after SOX, holding other factors constant.[17]

The control variables perform similarly across both regressions. As expected, the restatement earnings impact (*MAG*) is significantly related to returns, and the strength of this relation diminishes with large earnings impacts (i.e., the coefficient on $MAG * |MAG|$ is significantly negative). Returns are significantly more negative when the restatement lacks an initial magnitude disclosure (*NO_INITIAL_MAG*), involves long delays in releasing the magnitude (*LONG_MAG_DELAY*), and involves *FRAUD*, *CORE* items, or *MULTIPLE* errors. Unexpectedly, *QUARTERLY_ONLY* restatements are significantly associated with more negative announcement returns. Perhaps the market suspects that these restatements result from managers intentionally shifting earnings across

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[15] Huang (2010) reports that mean announcement returns for her restatement sample begin to decline in 2001. In contrast, in my sample, mean announcement returns (as opposed to median episode returns) do not exhibit a noticeable decline until 2003 (untabulated). The differing results may be due to differences in samples; Huang (2010) uses the Palmrose and Scholz (2004b) sample from 1995 to 2000 and the Audit Analytics sample from 2000 to 2006. The focus of my study is not to pinpoint when market reactions begin to decline; rather, I focus on the period when investors had many restatements to track, which begins in 2002.

[16] The *FRAUD* rate for pre-SOX restatements in my sample is lower than the near 50 percent rate that Hennes et al. (2008) report for their pre-SOX sample, but their sample consists of 1997 and 1998 only. In these years, my sample has a *FRAUD* rate of 47 percent, similar to Hennes et al.'s (2008).

[17] *POSTSOX* remains statistically and economically significant after excluding firms with low stock prices (less than $5 per share, two trading days prior to the restatement announcement), and excluding influential observations (observations for which the dfbetas statistic for *POSTSOX* has an absolute value higher than the $2 / (n^{1/2})$ cutoff recommended by Belsley et al. 1980). Because *FRAUD* is a crucial determinant of announcement returns (Hennes et al. 2008), I partition the sample into fraud and non-fraud observations and rerun the regressions. *POSTSOX* remains statistically and economically significant in both partitions.

TABLE 3
Tests of Announcement Returns

	Predicted Sign	Announcement Returns (*RETURN_ANNOUNCE*)	Episode Returns (*RETURN_EPISODE*)
Intercept	?	−0.0075	0.0157
		(0.0315)	(0.0494)
POSTSOX	?	0.0464 **	0.0597**
		(0.0098)	(0.0143)
Restatement Controls			
MAG	+	0.4160 *	0.9180**
		(0.1791)	(0.3104)
POSITIVE_MAG	+	0.0047	−0.0169
		(0.0076)	(0.0106)
MAG × *POSITIVE_MAG*	−	−0.1589	−0.7204
		(0.2427)	(0.4566)
MAG × |*MAG*|	−	−0.6567 *	−1.2072*
		(0.3857)	(0.6917)
NO_INITIAL_MAG	−	−0.0273 **	−0.0425**
		(0.0089)	(0.0109)
LONG_MAG_DELAY	−	−0.0197 *	−0.0364*
		(0.0097)	(0.0160)
FRAUD	−	−0.0518 **	−0.0756**
		(0.0105)	(0.0188)
FRAUD_LOWLEVEL	−	−0.0122	0.0024
		(0.0175)	(0.0275)
MGT_PROMPTS	+	−0.0060	−0.0002
		(0.0075)	(0.0113)
CORE	−	−0.0203 *	−0.0218*
		(0.0088)	(0.0126)
MULTIPLE	−	−0.0203 *	−0.0275*
		(0.0096)	(0.0162)
QUARTERLY_ONLY	+	−0.0407 **	−0.0581**
		(0.0092)	(0.0125)
General Controls			
RETURN_PRIOR	−	−0.0144	−0.0150
		(0.0120)	(0.0191)
FIRMSIZE	+	−0.0005	−0.0003
		(0.0019)	(0.0030)
VOLATILITY_INDEX	−	−0.0005	−0.0006
		(0.0006)	(0.0008)
ESURPRISE	+	0.2305	
		(0.3384)	
ESURPRISE_EPISODE	+		0.4022**
			(0.1610)
MBE	+	0.0335 **	
		(0.0087)	

(continued on next page)

TABLE 3 (continued)

	Predicted Sign	Announcement Returns (*RETURN_ANNOUNCE*)	Episode Returns (*RETURN_EPISODE*)
Adj. R^2		20.3%	22.4%
n Pre-SOX		407	352
n Post-SOX		819	716
n		1,226	1,068

*, ** Denote significance at the 5 percent and 1 percent levels, respectively (one-tailed if sign is in the predicted direction, two-tailed otherwise).

Models are estimated using OLS with heteroscedasticity-consistent standard errors (White 1980). Standard errors are presented in parentheses below coefficient estimates. The dependent variable is size-adjusted stock returns measured over either a "short" or "episode" window around the restatement announcement (*RETURN_ANNOUNCE* or *RETURN_EPISODE*).

See the Appendix for variable descriptions.

quarters to smooth trends or beat forecasts. The earnings surprise proxies are the only significant general control variables. The meet-or-beat indicator (*MBE*) is significantly related to short window returns, and the continuous measure of earnings surprises during the restatement episode (*ESURPRISE_EPISODE*) is significantly related to episode returns.[18]

Robustness of the *POSTSOX* Effect

In untabulated analysis, I test for nonlinearities involving *MAG* that might explain the observed *POSTSOX* effect by interacting *MAG* with the restatement characteristics that were statistically significant. The only statistically significant interaction is *MAG* * *FRAUD*; its positive coefficient suggests that the relation between returns and earnings impact is even stronger for fraudulent restatements. The coefficient on *POSTSOX* remains significantly positive, statistically and economically.

The post-SOX period contains four events that increased scrutiny on accounting reports, possibly leading to more technical restatements that spur less negative reactions. I include indicator variables to test whether the *POSTSOX* effect can be explained by restatements announced during these events: (1) re-audits of former Arthur Andersen clients after Andersen's collapse, (2) the first time managers had to certify their financial statements under SEC Order No. 4-460, (SEC 2002), (3) the first time managers had to certify their financial statements under SOX Section 302, and (4) the first time managers had to assess internal controls under SOX Section 404. The only statistically significant event indicator is the one related to SOX 404, and it is significant only in the short window regression. The *POSTSOX* coefficient remains statistically and economically significant in both regressions.

At this point, the analysis suggests that either investors under-react to post-SOX restatements, or that investor reaction is efficient but the model fails to control for changes in value-relevant factors after SOX. The next step is to determine whether prices drift negatively over the long run, which would indicate that the less negative initial reactions are under-reactions.

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[18] None of the regression inferences appear to be affected by collinearity. *MAG* and *MAG* * |*MAG*| have variance inflation factors (VIFs) around 12 and 10, which is not surprising given that the regression has several terms involving *MAG*. Der and Everitt (2002) consider VIFs higher than 10 cause for concern. Excluding *MAG* * |*MAG*| does not change inferences. All other variables have VIFs around 2 or below.

To focus the price drift tests on the subsets of restatements most likely to be mispriced, I use the episode return regression to determine whether particular types of restatements are responsible for the less negative post-SOX reactions. I interact *POSTSOX* with each independent variable one at a time, rerun the regression, and note which interactions reduce the coefficient magnitude on the *POSTSOX* intercept by two or more standard deviations. Based on this criterion, restatements involving *FRAUD*, long episodes (*LONG_MAG_DELAY*), and no initial earnings impact disclosure (*NO_INITIAL_MAG*) appear partially responsible for the less negative post-SOX reactions.

Long-Run Price Drift Results

I test for drift over six-month, one-year, and two-year horizons beginning at two different points: two days after the initial restatement announcement and two days after the earnings impact announcement. I use the earnings impact announcement as an alternative starting point because the initial restatement announcement often does not provide investors with full information about the restatement. I measure drift using restatement firms' median abnormal buy-and-hold return over the horizon, and consider the return statistically significant if it falls in the top or bottom 2.5 percentile of the empirical distribution of pseudo-portfolio median abnormal buy-and-hold returns. This procedure is essentially a two-tailed test conducted at the 5 percent level of significance.

Table 4, Panel A shows the distribution of the pseudo-portfolio median abnormal returns by SOX period at the three horizons. The means of the six-month, one-year, and two-year pre-SOX (post-SOX) distributions are −4.8, −11.4, and −17.1 percent (−3.5, −7.8, and −13.4 percent), respectively, indicating that negative bias exists in both SOX periods and is larger before SOX. The pseudo-portfolio approach controls for this negative bias because the ranking of the restatement portfolio return among the pseudo-portfolio returns determines statistical significance. In other words, if the restatement portfolio exhibits no drift other than that caused by the negative bias that affects all portfolios, then the restatement portfolio's drift would not fall in the extreme lower tail of the pseudo-portfolio distribution.[19]

Table 4, Panel B shows the median abnormal returns (i.e., drifts) of the restatement firms. Negative drifts in the post-SOX period would indicate that the less negative post-SOX announcement reactions documented in the previous section represent under-reactions to restatements. Panel B shows some evidence of negative drift following post-SOX restatements, but only when the starting point is immediately after the initial announcement (see second column of drifts); the full post-SOX sample has a significantly negative drift over the six months after the initial announcement (−5.3 percent), but not over one or two years. Turning to the post-SOX subsamples, restatements with long episodes (*LONG_MAG_DELAY*) have significantly negative drifts over the six months after the initial announcement (−7.5 percent), and fraudulent restatements have significantly negative drifts over the six months and one year after the initial announcement (−10.4 and −14.4 percent, respectively).[20] In contrast, the drifts beginning after the earnings impact announcement are smaller in magnitude and not statistically significant for the full post-SOX sample or subsamples (see fourth column). These findings suggest that the drifts beginning after the initial announcement are caused by firms slowly releasing restatement information rather than by investors mispricing released information.

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[19] Whereas the median pseudo-portfolio returns are negatively biased, the mean pseudo-portfolio returns are unbiased, i.e., when computing the *mean* abnormal return for each of the 1,000 pseudo-portfolios, the distribution of the 1,000 means has a mean near zero (untabulated). This indicates that the negative bias in the median pseudo-portfolio returns occurs because the median is not as affected by large positive returns among a relatively small number of firms. I focus on medians to limit the influence of these firms.

[20] I assess the significance of the drift in each subsample using pseudo-portfolios consisting of only the control firms matched to the restatement firms in each particular subsample.

TABLE 4

Tests of Long-Run Price Drifts

Panel A: Empirical Distributions of the 1,000 Pseudo-Portfolio Median Abnormal Buy-and-Hold Returns (cumulation begins after the restatement's earnings impact is announced)

				Percentiles					
	Mean	**Std. Error**	**Min**	**10th**	**25th**	**50th**	**75th**	**90th**	**Max**
Pre-SOX									
6 months	−4.8%	1.8%	−10.3%	−7.1%	−5.9%	−4.8%	−3.5%	−2.5%	1.4%
1 year	−11.4%	2.9%	−19.5%	−15.1%	−13.5%	−11.5%	−9.4%	−7.5%	−0.9%
2 years	−17.1%	3.9%	−31.1%	−21.9%	−19.5%	−17.1%	−14.4%	−12.3%	−3.8%
Post-SOX									
6 months	−3.5%	0.9%	−6.2%	−4.7%	−4.0%	−3.5%	−2.9%	−2.4%	−1.2%
1 year	−7.8%	1.4%	−12.2%	−9.7%	−8.7%	−7.8%	−6.9%	−6.0%	−4.1%
2 years	−13.4%	2.1%	−20.8%	−16.3%	−14.8%	−13.3%	−11.9%	−10.7%	−7.0%

Panel B: Median Abnormal Buy-and-Hold Returns for Restatement Firms

	Return Cumulation Begins Two Days After:			
	Initial Restatement Announcement		Earnings Impact Announcement	
	Pre-Sox	Post-SOX	Pre-SOX	Post-SOX
Full Sample				
Horizon				
6 months	**−16.1%****	**−5.3%***	**−11.6%****	**−4.4%**
1 year	**−27.9%****	**−8.4%**	**−22.1%****	**−6.7%**
2 years	**−39.3%****	**−11.9%**	**−38.7%****	**−11.1%**
n	422	792	416	783

(continued on next page)

Panel B: Median Abnormal Buy-and-Hold Returns for Restatement Firms

	Return Cumulation Begins Two Days After:			
	Initial Restatement Announcement		Earnings Impact Announcement	
	Pre-Sox	Post-SOX	Pre-SOX	Post-SOX
Sample Partitions				
No Initial Magnitude (*NO_INITIAL_MAG* = 1)				
6 months	−20.2%**	−4.9%	−11.2%**	−3.8%
1 year	−30.6%**	−8.1%	−20.9%**	−5.8%
2 years	−40.4%**	−9.9%	−39.4%**	−10.1%
n	212	478	206	469
Long Magnitude Delay (*LONG_MAG_DELAY* = 1)				
6 months	−21.8%**	−7.5%**	−9.3%	−4.9%
1 year	−30.1%**	−12.0%	−9.6%	−3.9%
2 years	−40.6%**	−16.3%	−33.1%**	−13.2%
n	131	293	125	284
Fraudulent (*FRAUD* = 1)				
6 months	−28.4%**	−10.4%**	−23.9%**	−5.7%
1 year	−30.8%**	−14.4%**	−25.1%**	−6.7%
2 years	−47.2%**	−14.7%	−45.5%**	−15.6%
n	130	161	124	153
Restatement Egregiousness Factor—Least Egregious Quartile				
6 months	−9.4%	−5.7%	−9.4%	−5.5%
1 year	−5.9%	−7.8%	−5.9%	−7.8%
2 years	−27.5%	−12.6%	−25.6%	−12.6%
n	98	194	98	194
Restatement Egregiousness Factor—Most Egregious Quartile				
6 months	−27.3%**	−11.7%**	−17.7%**	−6.2%
1 year	−35.3%**	−16.5%**	−22.0%*	−7.4%
2 years	−44.8%**	−20.2%	−43.8%**	−16.0%
n	118	181	112	173

(continued on next page)

Panel B: Median Abnormal Buy-and-Hold Returns for Restatement Firms

	Return Cumulation Begins Two Days After:			
	Initial Restatement Announcement		**Earnings Impact Announcement**	
	Pre-Sox	**Post-SOX**	**Pre-SOX**	**Post-SOX**
Announcement Returns—Most Positive Quartile				
6 months	−14.6%	−2.9%	−13.4%	−2.3%
1 year	**−33.3%****	**−5.4%**	−26.4%	−3.9%
2 years	−32.4%	−8.1%	−32.4%	−5.4%
n	85	212	84	212
Announcement Returns—Most Negative Quartile				
6 months	**−25.4%****	**−7.9%**	**−15.7%****	**−5.1%**
1 year	**−32.3%****	**−11.2%**	**−28.6%****	**−8.7%**
2 years	**−47.9%****	**−22.8%**	**−46.7%****	**−21.1%**
n	168	147	163	141
Unexplained Announcement Return—Highest Quartile (less negative initial reactions than expected)				
6 months	−15.4%**	−7.9%**	−12.7%*	−4.0%
1 year	**−35.7%****	**−12.3%***	**−23.9%***	**−7.6%**
2 years	**−35.9%****	**−9.0%**	**−32.4%***	**−6.1%**
n	87	169	86	165
Unexplained Announcement Return—Lowest Quartile (more negative initial reactions than expected)				
6 months	**−30.2%****	**−6.8%**	**−22.1%****	**−5.7%**
1 year	**−38.5%****	**−9.9%**	**−32.5%****	**−10.8%**
2 years	**−60.5%****	**−17.6%**	**−50.0%****	**−19.6%**
n	93	179	91	177

(continued on next page)

Panel C: Fama-French Abnormal Monthly Median Returns for Restatement Firms

	Return Cumulation Begins Two Days After:			
	Initial Restatement Announcement		Earnings Impact Announcement	
	Pre-Sox	Post-SOX	Pre-SOX	Post-SOX
Full Sample				
Horizon				
6 months	__−3.2%**__	__−0.7%*__	__−2.4%**__	__−0.8%**__
1 year	__−2.4%**__	__−0.6%*__	__−2.1%**__	__−0.5%**__
2 years	__−2.4%**__	__−0.5%*__	__−2.2%**__	__−0.5%**__
Sample Partitions				
No Initial Magnitude (*NO_INITIAL_MAG* = 1)				
6 months	__−4.9%**__	__−0.6%__	__−2.4%**__	__−0.8%*__
1 year	__−3.0%**__	__−0.5%*__	__−2.2%**__	__−0.5%*__
2 years	__−2.7%**__	__−0.5%**__	__−2.3%**__	__−0.5%**__
Long magnitude delay (*LONG_MAG_DELAY* = 1)				
6 months	__−5.0%**__	__−1.0%*__	−2.0%**	−1.2%*
1 year	−2.3%**	−0.8%*	−0.9%	−0.8%**
2 years	__−2.4%**__	__−0.7%**__	__−2.0%**__	__−0.6%**__
Fraudulent (*FRAUD* = 1)				
6 months	__−5.6%**__	__−1.4%*__	−2.6%*	−2.0%**
1 year	−2.9%*	−1.2%**	−2.1%*	−1.5%**
2 years	__−2.7%**__	__−1.2%**__	__−2.2%**__	__−1.0%**__
Restatement Egregiousness Factor—Least Egregious Quartile				
6 months	−0.8%	−1.2%**	−0.8%	−1.2%**
1 year	−1.2%	−0.8%**	−1.2%	−0.8%**
2 years	−1.4%**	−0.6%**	−1.4%**	−0.6%**
Restatement Egregiousness Factor—Most Egregious Quartile				
6 months	__−6.4%**__	__−1.3%*__	−2.6%**	−1.6%**
1 year	__−3.8%**__	__−1.3%**__	−1.9%*	−1.2%**
2 years	__−2.7%**__	__−1.0%**__	__−2.2%**__	__−0.9%**__
Announcement Returns—Most Positive Quartile				
6 months	−2.2%	−0.7%	−1.0%	−0.9%
1 year	−2.1%**	−0.5%	−1.8%*	−0.6%*
2 years	__−2.4%**__	__−0.5%*__	__−2.2%**__	__−0.5%*__

(continued on next page)

Panel C: Fama-French Abnormal Monthly Median Returns for Restatement Firms

	Return Cumulation Begins Two Days After:			
	Initial Restatement Announcement		**Earnings Impact Announcement**	
	Pre-Sox	**Post-SOX**	**Pre-SOX**	**Post-SOX**
Announcement Returns—Most Negative Quartile				
6 months	**−5.3%****	**−1.0%**	**−4.0%****	**−1.6%***
1 year	**−4.2%****	**−1.0%**	**−3.7%****	**−1.3%***
2 years	**−3.6%****	**−1.1%****	**−3.5%****	**−1.1%****
Unexplained Announcement Return—Highest Quartile (less negative initial reactions than expected)				
6 months	**−5.9%****	**−0.8%**	−2.9%*	−0.6%
1 year	**−3.1%****	**−0.5%**	−1.9%*	−0.2%
2 years	**−2.1%****	**−0.4%**	**−1.9%****	**−0.3%**
Unexplained Announcement Return—Lowest Quartile (more negative initial reactions than expected)				
6 months	**−5.0%****	**−0.8%**	**−4.5%****	**−1.7%****
1 year	**−4.0%****	**−0.9%***	**−3.5%****	**−1.2%****
2 years	**−3.4%****	**−1.1%****	**−3.3%****	**−1.3%****

Abnormal returns for each firm are computed by taking the firm's buy-and-hold return minus the buy-and-hold return of an equally weighted benchmark portfolio matched on size and book-to-market.

Panel A:

To form a single pseudo-portfolio, I match each restatement firm randomly with replacement to a non-restatement firm in the same size/book-to-market benchmark portfolio. Then I compute an abnormal return for each firm in the pseudo-portfolio as described above, and find the median abnormal return of the pseudo-portfolio. This process is repeated 1,000 times to form the empirical distributions of median returns shown above.

Panel B:

*, ** Denote that the value lies in the top or bottom 2.5 and 0.5 percentiles of the empirical distribution, respectively.

To identify a statistically significant difference from 0, I rank the median buy-and-hold abnormal return of the restatement firms within the empirical distribution of pseudo-portfolio returns. This corresponds to a two-tailed test at commonly accepted levels of significance. In each subsample, significance is assessed using pseudo-portfolios consisting only of the control firms matched to the restatement firms in that subsample.

To identify a statistically significant difference across SOX periods, I use an empirical distribution formed by computing the difference between the pre- and post-SOX median return for each pseudo-portfolio, and then rank the difference in median return across pre- and post-SOX restatement firms within the empirical distribution. Bold denotes that the difference between pre- and post-SOX returns lies in the top or bottom 2.5 percentile of the empirical distribution. This corresponds to a two-tailed test commonly accepted levels of significance.

Panel C:

(continued on next page)

*, ** Denote that the intercept statistically differs from 0 at the 5 percent and 1 percent levels, two-tailed, respectively. Bold denotes that the pre- and post-SOX intercepts statistically differ from each other at the 5 percent level, two-tailed.

Panel C reports the estimated intercepts from Fama and French (1993) calendar-time regressions of median monthly restatement firm returns on the three Fama-French risk factors. The regression is estimated using weighted least squares, with the weight corresponding to the number of firms in the restatement portfolio. Significance is assessed using heteroscedasticity-consistent standard errors (White 1980). To statistically assess the difference between pre- and post-SOX intercepts, I take the difference between the estimated intercepts and divide by the square root of the sum of the two intercepts' estimated variances, which is asymptotically standard normal assuming independent pre- and post-SOX samples.

I also rank restatements by egregiousness, announcement return, and unexplained announcement return, testing for drifts in the top and bottom quartiles of each variable. Restatement egregiousness is the score from a principle components analysis of the five restatement characteristics that were significant in the predicted direction in the announcement returns regression: *MAG*, *NO_INITIAL_MAG*, *LONG_MAG_DELAY*, *FRAUD*, and an indicator variable capturing whether the restatement involves *CORE* or *MULTIPLE* items.[21] I compute quartiles for the egregiousness factor and announcement return on the sample as a whole; they are not computed separately for pre- and post-SOX periods. For example, in order for a post-SOX restatement to be in the most egregious quartile, it must be among the most egregious of pre- and post-SOX restatements combined. I compute unexplained announcement return by running the short window announcement return regression (1) on the pre-SOX sample only, using the estimated coefficients to predict announcement returns for both the pre- and post-SOX samples, and computing the difference between actual and predicted.

For post-SOX restatements, the most egregious quartile and the quartile with the most positive unexplained announcement returns have significantly negative six-month and one-year drifts following the initial announcement. None of the quartiles have significant drifts following the earnings impact announcement. To summarize, the post-SOX period contains some evidence of negative drift following initial announcements, but no evidence of drifts once investors have relatively complete information about the restatements.

In contrast, the pre-SOX period contains strong evidence of negative drifts even after investors have relatively complete information about the restatements. For the full pre-SOX sample, drifts at six months, one year, and two years following the earnings impact announcement are −11.6, −22.1, and −38.7 percent, respectively, all significant at the 1 percent level (see third column). The negative drifts are consistent with investor under-reaction documented for many other types of public news events, including large quarterly earnings surprises, stock splits, stock repurchases, and dividend initiations and omissions (Daniel et al. 1998).

Comparing across periods, the negative drifts in the pre-SOX period are larger than those in the post-SOX period. The pairs of drifts that statistically differ across SOX periods are shown in bold in Panel B.[22] In the full sample, pre-SOX drifts are significantly more negative at all horizons. In the sample partitions, pre-SOX drifts are almost always more negative, and the difference is usually statistically significant. The pre-SOX drifts tend to be significantly more negative even in the quartiles of restatements that are the most egregious and that have the most negative initial reactions. This shows that the more negative pre-SOX drifts are not simply due to the more egregious nature of pre-SOX restatements; rather, investors appear to price even the most egregious restatements more efficiently after SOX.

Robustness Checks for Buy-and-Hold Drifts

Pre-SOX restatements may have more negative drifts because pre-SOX restatement episodes are shorter; i.e., in the pre-SOX period, firms are more likely to announce the restatement's earnings impact at the time of the initial announcement or shortly thereafter, giving investors less time to process and react to the information. Pre-SOX episodes are shorter than post-SOX episodes by a mean (median) of 13.5 (8.5) days. To investigate the role of shorter episodes, I compute a new set of drifts for pre-SOX restatements that do not begin until 17 days after the earnings impact announcement. Then I compare this set to the original set of post-SOX drifts that begin only two days after the earnings impact announcement. In the full sample, the pre-SOX drifts remain significantly negative and significantly more negative than the post-SOX drifts, and the same tends to be true of the sample partitions (untabulated).

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[21] The components load on a single factor using the Kaiser criterion—i.e., only the first factor has an eigenvalue greater than 1. The factor explains 45 percent of the total variation in the variables. The standardized scoring coefficients for *MAG*, *FRAUD*, *NO_INITIAL_MAG*, *LONG_MAG_DELAY*, and the *CORE* or *MULTIPLE* indicator are −0.194, 0.324, 0.333, 0.363, 0.240, respectively.

[22] To determine whether the difference in pre- and post-SOX drifts is statistically significant, I take each of the 1,000 pseudo-portfolios and subtract the pre-SOX median drift from the post-SOX median drift. This creates a distribution of 1,000 differences in pre- and post-SOX median drifts. Then I determine whether the difference in pre- and post-SOX median drift among restatement firms falls in the top or bottom 2.5 percentile of the empirical distribution. Note that this test assesses whether pre-SOX restatements have more negative drifts than post-SOX restatements after controlling for the larger negative bias that exists in pre-SOX drifts (Table 4, Panel A).

Next, I repeat the analysis of Table 4 using mean drifts instead of median drifts, and focus on the drifts beginning after the earnings impact announcement (untabulated). In both the pre- and post-SOX periods, the magnitudes of the drifts become smaller, and rarely significantly differ from zero or differ across SOX periods. In the full sample, the pre-SOX (post-SOX) drifts at six-month, one-year, and two-year horizons are −6.0, −7.8, and −2.1 percent (−0.6, 0.2, and 2.4 percent), respectively. None of these drifts are significantly different across SOX periods, and only the pre-SOX, six-month drift significantly differs from zero. The decrease in drift magnitude when using means indicates that a small number of firms have large positive drifts, which offset the negative drifts experienced by most of the restatement firms. In other words, the lack of significant mean drift hides the fact that, before SOX, investors typically under-reacted to restatements.[23]

I also test the influence of small firms, which are more likely to be mispriced because of illiquidity or low institutional following. I rerun the median and mean drift analyses using just the firms in the largest 9 of the 14 size partitions. The magnitudes and significance levels of the drifts are similar to those in the original analyses, and inferences are unchanged.

Fama-French Tests of Price Drifts

An alternative approach for detecting long-run price drifts is the Fama and French (1993) calendar-time regression. This approach tests whether monthly returns are persistently abnormal over the horizon, rather than whether the overall return for the horizon is abnormal (Lyon et al. 1999). The dependent variable for the Fama-French regression is a single time-series of monthly restatement portfolio returns. A firm enters the portfolio in the first calendar month following its restatement announcement and remains in the portfolio for 6, 12, or 24 months, depending on the testing horizon. To compute the portfolio return in each month t, I take the cross-sectional median of the returns of the firms in the portfolio (R_p), and subtract the one-month Treasury rate (R_f). I regress this monthly portfolio return on the three Fama-French risk factors, allowing the intercept (α) to capture the abnormal portion of the monthly return. The three risk factors are the value-weighted market return (R_m) less one-month Treasury rate, the return on a hedge portfolio of small and large stocks (SMB), and the return on a hedge portfolio of high and low book-to-market stocks (HML):[24]

$$R_{pt} - R_{ft} = \alpha + \beta_1(R_{mt} - R_{ft}) + \beta_2 SMB_t + \beta_3 HML_t + \varepsilon_t. \tag{5}$$

I estimate the model using weighted least-squares, with the weight corresponding to the number of firms in the restatement portfolio in month t, and use heteroscedasticity-consistent standard errors.

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[23] Not surprisingly, I find that both median and mean *unsigned* price drifts following the earnings impact date are higher before SOX, significantly so at all horizons for medians and at the 24-month horizon for means. I assess significance by computing the difference between pre- and post-SOX unsigned drift for each pseudo-portfolio, forming a distribution of 1,000 differences. Then I determine where the difference between pre- and post-SOX unsigned drift for the restatement portfolio lies in this distribution.

[24] I thank Kenneth French for providing the factors on his website: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/f-f_factors.html.

Table 4, Panel C shows estimates of monthly α over six-month, one-year, and two-year horizons. Focusing on the period beginning after the earnings impact date (columns 3 and 4), estimates of α tend to be significantly negative for both pre- and post-SOX restatements in the full sample and the partitions. This finding contrasts with the results for buy-and-hold returns, which were not significantly negative for post-SOX restatements. However, consistent with the buy-and-hold return results, pre-SOX α estimates are often significantly more negative than post-SOX α estimates.[25] In the full sample, pre-SOX α estimates over six-month, one-year, and two-year horizons are -2.4, -2.1, and -2.2 percent, and the corresponding post-SOX α estimates are -0.8, -0.5, and -0.5 percent, respectively. Each of these α estimates is significantly negative, and the pre-SOX α estimates are statistically more negative than the post-SOX α estimates at all horizons. Converting these monthly α estimates to accumulated six-month, one-year, and two-year median returns, the results are -14.4, -25.2, and -52.8 percent for pre-SOX restatements and -4.8, -6.0, and -12.0, respectively, for post-SOX restatements.[26] The α estimates for the sample partitions exhibit similar patterns. In summary, both the buy-and-hold and Fama-French results for median returns suggest that investors typically price restatements more efficiently after SOX, despite the larger number of restatements to process.

For robustness, in untabulated tests I estimate α using mean instead of median monthly restatement firm returns. Similar to the results for mean buy-and-hold returns from the previous section, estimates of α tend to be statistically insignificant from zero and never significantly differ across SOX periods. I also rerun the Fama-French analyses after restricting the sample to firms in the largest 9 of the 14 size partitions, and find results similar to those of the full sample.

Volume Results

Even if investor confusion does not cause mispricing, confusion could lead to disagreements among investors that spur more trading volume relative to contemporaneous price changes. Table 5 shows the results of regressing abnormal trading *VOLUME* in the three days around the restatement announcement on *POSTSOX* and control variables. The *POSTSOX* coefficient is positive, as predicted, but not statistically different from zero, providing no reliable evidence of increased investor disagreement after SOX. The coefficient on absolute abnormal return (*AB_RETURN*) is significantly positive, confirming that volume increases in the news of the announcement. The interaction between *AB_RETURN* and *POSTSOX* is insignificant, suggesting that the relation between volume and absolute return is similar across the two SOX periods. Because *POSTSOX* appears in the regression as a main term and as an interaction with *AB_RETURN*, I also evaluate the combined marginal effect of the *POSTSOX* terms on volume when *AB_RETURN* is at its mean. The combined effect of the *POSTSOX* terms is negative and insignificant, contrary to predictions of higher volume in the post-SOX period.[27]

Turning to the remaining control variables, the *AB_RETURN* * *POSITIVE_RETURN* interaction is significantly negative, suggesting that volume is not as responsive to restatement news

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[25] To statistically assess the difference between pre- and post-SOX α estimates, I take the difference between the α estimates and divide by the square root of the sum of the two estimated α variances, which is asymptotically standard normal, assuming independent pre- and post-SOX samples.

[26] To convert to an accumulated return, I multiply the six-month α by 6, the one-year α by 12, and the two-year α by 24.

[27] The combined effect is -0.002, computed as: 0.0032 *POSTSOX* coefficient + (-0.0553 interaction coefficient x 0.094 mean *AB_RETURN*). The *POSTSOX* main effect and combined effect remain statistically insignificant in two alternative specifications. The first specification limits the sample to firms that have stock prices of at least $5 per share. The second specification does not adjust the dependent variable for the firm's normal level of volume, i.e., the specification uses only $TO_{restate,i}$ from Equation (4) as the dependent variable. I run this second specification because if investors are more confused about poorly performing firms, then adjusting for the firm's normal level of volume could hide the effect of interest. In all specifications, multicollinearity does not appear to affect inferences, as variance inflation factors are low.

TABLE 5
Tests of Volume around Restatement Announcements

		Dependent Variable
	Predicted Sign	***VOLUME***
Intercept	?	−0.0397**
		(0.0090)
POSTSOX	+	0.0032
		(0.0036)
Restatement Controls		
AB_RETURN	+	0.1929**
		(0.0299)
AB_RETURN × *POSTSOX*	?	−0.0553
		(0.0319)
POSITIVE_RETURN	?	0.0068**
		(0.0019)
AB_RETURN × *POSITIVE_RETURN*	?	−0.1298**
		(0.0172)
AB_MAG	+	0.0118
		(0.0157)
POSITIVE_MAG	−	0.0019
		(0.0020)
NO_INITIAL_MAG	+	0.0015
		(0.0019)
LONG_MAG_DELAY	+	−0.0029
		(0.0027)
FRAUD	+	0.0065**
		(0.0028)
FRAUD_LOWLEVEL	+	−0.0083**
		(0.0027)
MGT_PROMPTS	−	−0.0012
		(0.0019)
CORE	+	0.0034*
		(0.0021)
MULTIPLE	+	0.0025
		(0.0021)
QUARTERLY_ONLY	−	−0.0001
		(0.0025)
General Controls		
AB_RETURN_PRIOR	−	0.0022
		(0.0067)
FIRMSIZE	?	0.0026**
		(0.0005)
VOLATILITY_INDEX	+	−0.0002*
		(0.0001)
AB_ESURPRISE	+	0.0684
		(0.0629)
MBE	?	0.0031
		(0.0021)

(continued on next page)

TABLE 5 (continued)

	Predicted Sign	Dependent Variable *VOLUME*
Adj. R^2		33.8%
n Pre-SOX		407
n Post-SOX		819
n		1,226

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*, ** Denote significance at the 5 percent and 1 percent levels, respectively (one-tailed if sign is in the predicted direction, two-tailed otherwise).

Models are estimated using OLS with heteroscedasticity-consistent standard errors (White 1980). Standard errors are presented in parentheses below coefficient estimates. The dependent variable is abnormal *VOLUME* over days (−1, +1) relative to the restatement announcement.

See the Appendix for variable descriptions.

when the news is good. Restatements involving *FRAUD* and *CORE* items are significantly associated with higher volume, although restatements that involve fraud at lower levels of the firm (*FRAUD_LOWLEVEL*) are significantly associated with lower volume. Volume is also significantly increasing in *FIRMSIZE* and decreasing in the implied level of market volatility (*VOLATILITY_INDEX*).

VI. CONCLUSION

Pricing and volume tests provide little evidence that investors are confused by post-SOX restatements. Initial reactions to post-SOX restatements are less negative than those of pre-SOX restatements even after attempting to control for the less egregious nature of post-SOX restatements. While this finding could be consistent with investor confusion, there is little evidence of negative long-run price drifts unique to the post-SOX period, which would occur if the less negative initial reactions represented under-reactions by confused investors. Far from appearing confused, that market appears to have become significantly more efficient at pricing restatements after SOX, as the price drifts are often smaller after SOX than before.

Prices reflect the aggregate judgment of investors. Although aggregate judgment appears to improve after SOX, it is possible that confusion among individual market participants could lead to more disagreements over valuations. I test for increased disagreement by examining trading volume incremental to price movements around restatement announcements, but do not find evidence of an increase in disagreement after SOX.

These results alleviate concerns that the dramatic increase in restatements after SOX has confused investors. However, there may be other reasons to curtail restatements, which future research can address. While this study focuses on the capital market consequences for the restating firms themselves, the flood of restatements after SOX could affect public confidence in the capital markets as a whole. Future studies could examine macro-level effects of restatements, or other concerns of regulators such as the cost to preparers.

From a standard-setting perspective, the results suggest that investors were able to adapt as more firms revised financial statements to correct errors. To better understand investors' capacity to process other types of financial statement revisions, future research could examine revisions due to changes in accounting principle or changes in reporting entity. In 2005, firms began revising prior financial statements for changes in accounting principle, so evidence on how investors adapted would help standard-setters decide whether to expand retrospective treatment to more types of accounting changes.

APPENDIX
VARIABLE DESCRIPTIONS

Panel A: Dependent Variables

RETURN_ANNOUNCE	Size-adjusted buy-and-hold stock return over days (−1, +1) relative to the initial restatement announcement, including any CRSP delisting return. Winsorized at 1 and 99 percent.
RETURN_EPISODE	Size-adjusted buy-and-hold stock return from one day before the initial restatement announcement to one day after the announcement of the restatement's earnings impact, including any CRSP delisting return. Winsorized at 1 and 99 percent.
VOLUME	Abnormal volume over days (−1, +1) relative to the initial restatement announcement, computed similarly to Garfinkel and Sokobin (2006).

Panel B: Explanatory Variable of Interest

POSTSOX	Equals 1 if the restatement is announced in the month SOX was passed (July 2002) or after, 0 otherwise.

Panel C: Other Explanatory Variables

AB_ESURPRISE	Absolute value of *ESURPRISE.*
AB_MAG	Absolute value of *MAG.*
AB_RETURN_PRIOR	Absolute value of *RETURN_PRIOR.*
AB_RETURN	Absolute value of *RETURN_ANNOUNCE.*
CORE	Equals 1 if the restatement affects components of pre-tax operating income other than non-cash items like depreciation, amortization, and equity-based compensation, 0 otherwise.
ESURPRISE	If earnings are announced in the three-day window around the restatement announcement, then *ESURPRISE* is the difference between actual earnings per share and the most recent consensus analyst forecast from I/B/E/S, scaled by stock price one day before the earnings announcement. Winsorized at 1 and 99 percent. If earnings are not announced in the three-day window around the restatement announcement, then *ESURPRISE* equals 0.
ESURPRISE_EPISODE	The sum of all the individual *ESURPRISEs* in the restatement episode window. The consensus forecast for the first *ESURPRISE* after the restatement announcement is taken immediately after the restatement announcement. The consensus forecasts for subsequent *ESURPRISEs* are taken immediately after the previous *ESURPRISE.* Winsorized at 1 and 99 percent.
FIRMSIZE	Natural log of the firm's market capitalization two days before the restatement announcement.
FRAUD	Equals 1 if the errors are described as intentional or if investigations by a government entity, the board of directors, or audit committee are made public (Hennes et al. 2008). Equals 0 otherwise or if the fraud was committed in lower levels of the organization.
FRAUD_LOWLEVEL	Equals 1 if a fraud or investigation pertains to errors committed in subsidiaries or lower levels of the organization, 0 otherwise.
LONG_MAG_DELAY	Equals 1 if more than 30 days elapse between the initial restatement announcement and the announcement of the restatement's definitive earnings impact, 0 otherwise.
MAG	Cumulative impact of the restatement on past earnings, scaled by total assets for the fiscal year ended prior to the restatement announcement. Winsorized at 1 and 99 percent.
MBE	Equals 1 if earnings are announced in the three-day window around the restatement announcement and they meet or beat the most recent consensus analyst forecast on I/B/E/S, 0 otherwise.

(continued on next page)

Panel C: Other Explanatory Variables

MGT_PROMPTS	Equals 1 if the GAO report identifies the company as the prompter of the restatement (as opposed to the auditor or the SEC), 0 otherwise.
MULTIPLE	Equals 1 if the restatement spans item categories or involves three or more errors in the same category, 0 otherwise. The categories are (1) CORE income items, (2) depreciation, amortization, equity-based compensation, and other non-cash components of pre-tax operating income, (3) three common operating lease accounting errors announced by many firms in 2004 and 2005 (Acito et al. 2009), (4) taxes, (5) derivatives, (6) impairments and other valuation issues associated with noncurrent assets and liabilities, (7) off-balance-sheet obligations, (8) financing activities, (9) balance sheet or statement of cash flow reclassifications not affecting income, (10) merger-related or special items not included in other categories, and (11) unspecified or other errors.
NO_INITIAL_MAG	Equals 1 if no definitive earnings impact is disclosed when the initial restatement announcement is made, 0 otherwise.
POSITIVE_MAG	Equals 1 if cumulative restated earnings are greater than or equal to cumulative original earnings, 0 otherwise.
POSITIVE_RETURN	Equals 1 if *RETURN_ANNOUNCE* > 0, 0 otherwise.
QUARTERLY_ONLY	Equals 1 if the restatement involves only prior quarters of the current fiscal year, 0 otherwise.
RETURN_PRIOR	Size-adjusted buy-and-hold stock return over days $(-180, -2)$ relative to the initial restatement announcement. Winsorized at 1 and 99 percent.
VOLATILITY_INDEX	Mean value of the Chicago Board Options Exchange's volatility index over the three-day or episode return windows.

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Asset Securitization, Securitization Recourse, and Information Uncertainty

Mei Cheng
The University of Arizona

Dan S. Dhaliwal
The University of Arizona and Korea University Business School

Monica Neamtiu
The University of Arizona

ABSTRACT: In this study, we examine some of the consequences of asset securitization. Specifically, using a sample of bank holding companies, we investigate whether the difficulty in assessing the true extent of risk transfer, between securitizing banks and investors in asset-backed securities, affects bank information uncertainty. We find that when market participants have a greater difficulty in estimating risk transfer, banks face greater information uncertainty (i.e., larger bid-ask spreads and analyst forecast dispersion). In addition, we find that this effect is mitigated for banks that operate in a higher quality information environment. We also find that banks that securitize financial assets have higher spreads and analyst forecast dispersion as compared to non-securitizing banks.

Keywords: *securitization; recourse; information uncertainty; banks.*

JEL Classifications: *G14; G21.*

I. INTRODUCTION

The volume of securitized financial assets has increased considerably over the last two decades. According to the Bond Market Association, as of the end of the first quarter of 2007, the asset-backed market comprised roughly \$8.9 trillion in outstanding securities compared to \$5.4 trillion in outstanding corporate bonds and about \$4.5 trillion in outstanding U.S. Treasury debt.

The rapid growth in securitization activities and the recent turmoil affecting securitization markets raise interesting questions regarding the benefits and costs of this type of financing

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mechanism. Prior academic and practitioner literature focuses primarily on the incentives and benefits of securitizing financial assets (e.g., Ryan 2007; Clarkson et al. 2002). This study extends prior research by examining some of the consequences of asset securitization. Specifically, we investigate the extent to which the difficulty in assessing the extent of risk transfer from securitizing banks to investors in asset-backed securities is associated with measures of bank information uncertainty.[1] Assessing the true extent of risk transfer is a critical issue in the securitization context and a significant focus of both academic research and standard-setters (Schipper and Yohn 2007).

We hypothesize that the difficulty in assessing the extent of recourse is positively associated with securitizing banks' information uncertainty. We expect market participants to have difficulty assessing the true extent of securitization recourse because of the complexity and lack of transparency associated with asset securitization transactions.[2] In practice, securitization transactions can have very complex structures. Banks typically rely on multiple special purpose entities (SPEs) to structure these transactions. In addition, various securitization structures can differ substantially in the extent to which issuers retain the risks associated with the securitized assets, ranging from a minimum to almost complete risk transfer. More importantly, investors and financial analysts have repeatedly indicated that the amount of securitization disclosure is insufficient and greater transparency is needed to fully understand the extent of risk transfer associated with securitized financial assets (FASB 2008; CFA Institute 2008). The difficulty in estimating risk transfer is likely to lead to information asymmetry among market participants if some market participants have better information and/or better information-processing abilities (Barth et al. 2003). It is also possible that the difficulty in assessing securitization risk transfer generates disagreement even among investors with similar levels of expertise and sophistication (Schwarcz 2004).

Because market participants' uncertainty with respect to the extent of risk transfer is not directly observable, in our empirical analyses we select several measures we expect to be correlated with the uncertainty about the extent of recourse. We use banks' securitization volume, securitization income, the proportion of nonperforming loans, charge-offs associated with securitized loans, and the volume of asset-backed securities retained by securitizing banks on their balance sheets. To mitigate the concern that each of these individual proxies measures the underlying concept of interest (i.e., the uncertainty about the extent of risk transfer) with error, we use principal component analysis and construct a recourse uncertainty factor based on these measures. We test our hypothesis by regressing proxies for bank information uncertainty, such as bid-ask spreads and earnings forecast dispersion, on the recourse uncertainty factor and control variables for a sample of securitizing banks. Our tests consistently document a positive association between the recourse uncertainty factor and banks' bid-ask spreads and analyst forecast dispersion.

In light of these findings, we also investigate whether the quality of the information environment (i.e., the quality of relevant information and the degree of investor sophistication) impacts the relation between the difficulty in estimating recourse and a securitizing bank's information uncertainty. Cross-sectional tests about how the effect of recourse uncertainty varies with the quality of the information environment can also help mitigate the concern that, despite controlling for risk measures frequently used in prior literature, our recourse uncertainty proxy simply captures the effect of securitization on bank intrinsic risk and not the effect on bank information uncertainty. If our recourse uncertainty measure captures the information effect of securitization, then we hypothesize that the impact of the difficulty in estimating recourse on bank information uncertainty will be mitigated for banks that operate in a higher quality information environment.

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[1] In the context of this study, by "greater information uncertainty" we mean lower precision of financial information and/or greater information asymmetry among information users. The information uncertainty is equivalent to the estimation risk (i.e., uncertainty in the parameters of the model or parameter uncertainty) discussed in the prior literature (Klein and Bawa 1976; Barry and Brown 1985). Empirically, it is the residual uncertainty after controlling for conventional risk measures. Prior literature (e.g., Barry and Brown 1985; Clarkson et al. 1996; Easley and O'Hara 2004; Amihud and Mendelson 1986, 1989) shows that greater information uncertainty can have real economic costs in the form of a higher cost of capital.

[2] In this study, we broadly refer to risk retained through both contractual obligations (i.e., retained interests) and noncontractual promises (i.e., implicit support) as recourse. For a detailed discussion about the importance of recourse in securitization transactions, see the "Background and Related Literature" and "Hypotheses Development" sections.

When we include in our main regressions interaction terms between the recourse uncertainty measure and the quality of the information environment proxies (i.e., analyst following, institutional investor holdings, and bank size), the findings support our hypothesis that the effect of recourse on banks' bid-ask spread and forecast dispersion is mitigated for banks with larger analyst following, larger institutional investor holdings, and greater size. In addition, when we interact the recourse uncertainty measure with information environment and intrinsic risk proxies, our information environment results continue to hold, while the impact of intrinsic risk on the relation between recourse uncertainty and bid-ask spreads/forecast dispersion is not statistically significant.

Given our finding that more difficulty in assessing risk transfer for securitizing banks is positively associated with greater bank information uncertainty, an interesting related question is whether securitization results in banks that engage in this type of financing transactions being more or less exposed to information uncertainty, as compared to banks that do not securitize assets. Securitization transactions can impact information uncertainty through several mechanisms. On the one hand, compared to non-securitizing banks, securitizing banks are exposed to uncertainty about the true level of risk transfer associated with the transferred assets and uncertainty about adequate reinvestment opportunities for the large cash amounts generated through securitization. On the other hand, securitization could lead to lower information uncertainty for securitizing banks relative to non-securitizing banks because of additional disclosure requirements for securitized assets.[3] We find that, after controlling for other determinants of information uncertainty, banks that securitize financial assets have higher spreads and analyst forecast dispersion as compared to banks that do not engage in securitization. These results are robust to controlling for potential endogeneity bias.

This study contributes to the literature by investigating the effect of the difficulty in assessing recourse (a key securitization feature) of bank information uncertainty. Many of the prior securitization studies (e.g., Niu and Richardson 2006; Landsman et al. 2008; Chen et al. 2008) examine whether, in their assessments of firm systematic risk, investors treat securitizations as a form of secured borrowing. They find that, *on average*, market participants view off-balance sheet securitized assets as having (to an extent) similar risk and value relevance as the assets kept on the issuer's balance sheet. In contrast, we follow the suggestion for future research issued by Schipper and Yohn (2007) to "provide additional insight into other investor judgments and decisions" related to securitization. Our study focuses on how market participants *differ* in their understanding and estimation of the degree of risk transfer (i.e., how market participants' difficulty in assessing the level of risk transfer affects the information uncertainty of securitizing banks).

In addition, our findings suggest that during our sample period, at least for some banks, the required disclosures did not provide enough information for market participants to sufficiently understand the level of securitization recourse. Our results also indicate that the impact of uncertainty about the level of risk transfer on bank information uncertainty can be mitigated for securitizing banks that operate in a high-quality information environment. Therefore, this study should be of interest to accounting regulators because it highlights the importance of high-quality disclosure in understanding the impact of securitization risk transfer. These findings support the FASB's concerns that companies provided insufficient and hard-to-interpret securitization disclosures (FASB 2002, 2008) and the FASB's recent efforts to improve such disclosures. Future research can examine whether the recently enhanced disclosures for securitized assets (ASC 860-10, formerly SFAS No. 166, FASB 2009a, 2009b) mitigate the impact of securitization recourse on firms' information uncertainty.

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[3] When issuing asset-backed securities, securitizing banks are required to disclose more information about these assets (through the process of registering the asset-backed securities with the SEC) compared to non-securitizing banks that keep the loans on the balance sheet (Schwarcz 2004; Foley et al. 1999). The additional required disclosure can lead to reduced information uncertainty with respect to not only the securitized assets, but also to the securitizing firms in general (Foley et al. 1999; Berger and Udell 1995; Cantor and Rouyer 2000).

The next section provides background information. Section III develops our hypotheses. Section IV presents our sample selection and descriptive statistics. Section V discusses the research design and the findings. Section VI concludes the study.

II. BACKGROUND AND RELATED LITERATURE

Accounting Treatment

Under ASC 860 (formerly SFAS No. 140, FASB 2000), securitizations can be accounted for as either sales or secured borrowings. For a transfer of financial assets to an SPE to qualify for sale accounting treatment, it must meet the following criteria: (1) the assets are isolated from the transferor and its creditors even in bankruptcy; (2) the SPE has the right to pledge or exchange the assets; and (3) the transferor does not maintain effective control over the assets through certain forms of continuing involvement.[4] If the securitization receives sale accounting treatment, then the transferor: (1) removes the assets from its balance sheet; (2) records cash proceeds in the amount received and recognizes any noncash proceeds in the securitized assets at fair value; (3) recognizes retained asset-backed securities at the book value of the securitized assets times the fair value of the retained securities divided by the fair value of the securitized assets; (4) recognizes retained contractual interests other than asset-backed securities, (e.g., servicing assets and recourse liabilities) at fair value; and (5) records a gain or loss on the sale to balance the journal entry. If the securitization is accounted for as a sale and additional conditions are satisfied (see Gorton and Souleles 2006), then the issuer does not have to consolidate the assets and liabilities held by its SPEs. If the transfer is accounted for as secured borrowing, then the financial assets remain on the balance sheet and the issuer recognizes a liability for the proceeds from the transfer.

Securitization Recourse

In a securitization transaction, the securitizing bank transfers pools of assets (such as mortgages or trade and credit card receivables) to SPEs. The SPEs finance these assets by selling to investors various classes of securities backed by the transferred asset pools and the cash flows they generate. The securities issued in this manner are commonly referred to as asset-backed securities. Investors in asset-backed securities face information asymmetry stemming from securitizing banks having better information about the transferred assets than do they. To mitigate potential adverse-selection problems, the securitizing bank offers to the outside investors some form of recourse. In the context of securitization transactions, recourse usually referrers to guaranties promised to investors in asset-backed securities allowing the transfer of some losses back to the originating bank if the performance of the underlying pools of securitized assets deteriorates.

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[4] In practice, banks typically structure securitization transactions by transferring pools of assets to qualified special purpose entities (QSPEs). QSPEs are exempt from consolidation requirements under ASC 860 (formerly SFAS No. 140), the accounting standard that covers securitization transactions during our sample period. In 2009, the FASB issued two new accounting standards, ASC 860-10 and ASC 810 (formerly SFAS No. 166 and SFAS No. 167, respectively) that determine whether securitizations and other transfers of financial instruments are given off-balance sheet treatment. The revisions to the securitization standards will result in many existing off-balance sheet securitizations being treated as secured financing and added back on-balance sheet.

Recourse can take an explicit form. For example, in many securitization transactions, the securitizing banks retain on their balance sheet the most subordinated asset-backed securities (often referred to as retained interests) that bear the first loss from the pool of securitized assets. The banks sell to outside investors the senior asset-backed securities.

To maintain their reputation and future access to securitization markets, issuers can also offer an implicit (i.e., non-contractual) promise to support underperforming securitized assets. Loss of reputation (i.e., the ability to sell loans economically) can expose banks to decreased liquidity, increased interest rate risk, and potential burdensome regulatory supervision. An originating bank can provide implicit support in various ways. In 2002, the Federal Financial Institutions Examination Council (FFIEC) released a document to assist in identifying cases of implicit recourse. The guidance lists four major actions that signal possible implicit recourse: (1) selling assets to a securitization trust or other SPE at a discount from the price specified in the securitization documents, which is typically par value; (2) purchasing assets from a trust or other SPE at an amount greater than fair value; (3) exchanging performing assets for non-performing assets in a trust or other SPE; and (4) funding credit enhancements beyond contractual requirements.[5]

Anecdotal evidence and academic research (Calomiris and Mason 2004; Higgins and Mason 2004; Gorton and Souleles 2006; Vermilyea et al. 2008) indicate that many issuers have chosen to implicitly support troubled transactions rather than impose credit losses on investors in asset-backed securities. The academic literature on implicit recourse focuses both on reasons for its existence and the effects. Gorton and Souleles (2006) provide theoretical arguments that the existence of implicit recourse is meant to reduce moral hazard problems. In the context of credit card securitizations, Calomiris and Mason (2004) find that this implicit recourse is more in line with an optimal contracting view of securitization rather than an attempt to reduce regulatory capital requirements and avoid early amortization. Prior literature also documents that recourse provides some benefits to the originating banks, including improved short- and long-term stock price performance and financial performance (e.g., Higgins and Mason 2004). In contrast, our study emphasizes the consequences for the originating banks stemming from market participants' difficulty in assessing the true level of securitization recourse.

Recent literature (e.g., Mason 2009) discusses the sources of information asymmetry that are related to recourse, including not knowing the distribution of external shocks among various classes of securitized assets, the lack of understanding about the triggers and terms of securitization recourse, and the difficulty in estimating the impact of recourse. Mason (2008) even suggests that, given the large amount of asymmetric information in poor securitization disclosure, it is not surprising that a crisis occurred once securitization reached a sufficient scale. Our study extends this line of literature by empirically measuring recourse uncertainty and examining its information consequences at the securitizing bank level.

III. HYPOTHESES DEVELOPMENT

Because of the possibility of recourse, a critical issue in understanding the impact of securitizing assets on banks that originate such transactions is the extent to which the credit risk associated with the securitized assets has been transferred from the originating bank to those who invest in asset-backed securities. To understand the effect of recourse on the securitizing banks, market participants have to estimate: (1) the anticipated need for future support for troubled securitization transactions (i.e., expected future losses associated with securitized assets) and (2) banks' willingness and potential methods of offering support, if the need arises.

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[5] These four FFEIC actions can signal implicit recourse, but their presence alone is not sufficient to definitely conclude the existence of implicit recourse. Regulators that want to prove a failure to consolidate in the presence of implicit recourse would have to further investigate each particular case and its specific details. In addition, implicit recourse can also take forms that are more subtle than the four FFEIC events listed above. For example, anecdotal evidence indicates that banks sometimes misclassify certain credit losses as fraud losses to hide the fact that they did offer implicit recourse.

Market participants are likely to find it difficult to assess the true extent of securitization recourse because securitization transactions are very complex. In practice, various features of securitization transactions (such as the magnitude of retained asset-backed securities, the level of subordination of retained interests, the credit enhancement offered) and thus, the extent of risk transfer, vary considerably even across securitization transactions originated by the same bank. As for implicit recourse obligations, information users find it difficult to even identify (let alone measure) such obligations, because banks may or may not support their troubled securitizations at their discretion.

Note that the complexity and heterogeneity of securitization transactions would not lead to uncertainty with respect to the true extent of securitization risk transfer if publicly available securitization-related disclosure is adequate or market participants are able to use their private sources of information to clearly assess the extent of risk transfer. However, there are reasons to believe that the quality of securitization-related disclosure is inadequate. Financial statement users repeatedly indicate that the amount of securitization disclosure is insufficient and greater transparency is needed to fully understand the extent of risk transfer associated with securitized financial assets (FASB 2008). Further, financial analysts surveyed by the CFA Institute in 1999, 2003, and 2007 consistently rated securitization-related disclosure as high in importance, yet assigned low ratings for the quality of such disclosure (CFA Institute 2008).

In the securitization context, the difficulty in estimating risk transfer can lead to information asymmetry and disagreement among market participants if some market participants have better information and/or better information-processing abilities. Prior studies (Ryan 2007; Schwarcz 2004) argue that financial reporting requirements simplify considerably the underlying economics behind asset securitizations and, therefore, cannot fully describe these complex transactions. Although the risk transfer associated with securitized assets varies on a continuum, the financial reporting of securitizations offers only a simplified dichotomous choice (i.e., sale versus secured borrowing accounting).[6] In practice, the majority of securitization transactions are recorded using sale accounting and the transactions are kept off-balance sheet. Because of this financial reporting treatment, information users have to rely on footnotes to the financial statements and other disclosure sources. Prior literature (e.g., Barth et al. 2003) suggests that reliance on disclosed rather than recognized information leads to information asymmetry among market participants, insofar as investors with a higher level of expertise are able to collect information from disclosures at a lower cost and/or a faster pace, thus obtaining an informational advantage over investors with less expertise. Mason (2009) discusses some specific sources of information asymmetry related to securitization recourse, such as not knowing the distribution of external shocks among various classes of securitized assets, lack of understanding about the triggers and terms of securitization recourse, and difficulty in estimating the impact of recourse on the originating bank. In addition, difficulty in assessing the extent of recourse can generate disagreement even among investors with similar levels of expertise and sophistication. Some authors (e.g., Schwarcz 2004) suggest that securitization transactions are so complex that the sophistication of their disclosure provides a level of sophistication that is challenging even for institutional investors and security analysts. Therefore, we hypothesize that measures of difficulty in assessing the extent of recourse are positively associated with securitizing banks' information uncertainty.[7]

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[6] Under U.S. GAAP, sale accounting is permitted when a firm surrenders control over the securitized assets (see Section II for a more detailed discussion of this point).

To be sure, some banks have a higher quality information environment (i.e., a higher quality of relevant information and degree of investor sophistication). For securitizing banks that operate in a higher quality information environment, market participants have better information about investment opportunities, lending standards, alternative sources of financing, and bank reputation concerns. This type of information is critical in anticipating a bank's need and willingness to provide future support for its troubled securitization transactions. In addition, for securitizing banks that have a greater percentage of institutional ownership, investors have a higher level of expertise on which to rely when collecting and processing information about complex securitization transactions. Therefore, we hypothesize that the impact of the difficulty in estimating recourse on bank information uncertainty will be mitigated for banks that operate in a higher quality information environment. These cross-sectional predictions about how the effect of recourse uncertainty varies with the quality of the information environment can also help mitigate the concern that, despite controlling for risk measures frequently used in prior literature, our recourse uncertainty analysis simply captures the effect of securitization on bank intrinsic risk and not the effect on bank information uncertainty.[8]

IV. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

In testing our hypotheses, we focus on the impact of the difficulty in estimating securitization recourse on the level of information uncertainty in securitizing banks. While other types of firms securitize financial assets as well, we choose to investigate the loan securitizations of banks for several reasons. First, given that banks operate in a regulated industry, their financial reporting of securitizations is substantially more homogenous than the reporting of firms in various other industries. Therefore, focusing on bank securitizations allows us to reduce the impact of variations in the regulatory disclosure level on bank information uncertainty. Second, by focusing on the banking industry, we mitigate data availability concerns. In contrast to banks, firms in other industries vary considerably more in the degree to which they present comprehensive securitization information. Third, banks are major participants in securitization markets. Prior studies (e.g., Dechow et al. 2010) that use a multi-industry sample find that only 30 percent of their sample firms come from nonfinancial industries and banks are the largest category among the financial firms engaged in securitizations.

We start our sample selection by identifying all bank holding companies (hereafter, banks) with the necessary quarterly financial information available from the regulatory Y-9C reports that any bank with consolidated assets of at least $150 million ($500 million after March 2006) must file with the Federal Reserve.[9] We eliminate banks whose common equity is not traded on the NYSE, NASDAQ, or AMEX stock exchanges, as one of our information uncertainty measures (i.e., the effective bid-ask spread) is market-based. From the Y-9C reports, we gather bank-quarter information from 2001 (second calendar quarter) to 2007 (second calendar quarter). We identify banks that securitize assets and collect information about our securitization variables from schedule HC-S (Servicing, Securitization, and Asset Sale Activities) of the Y-9C reports.[10] Our sample starts with the second quarter of 2001 because the Schedule HC-S was first available then. We end our sample period with the second quarter of 2007 because we are interested in studying the impact of securitization transactions on information uncertainty under normal market conditions (i.e., before the current securitization crisis). The securitization transactions reported on the Schedule HC-S are accounted for as sales. Consistent with prior research (e.g., Chen et al. 2008; Landsman et al. 2008), we focus on securitizations accounted for as sales, because typically the characteristics of securitizations accounted for as secured borrowings are unobservable to bank outsiders.

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[7] However, one may conceive that, for some banks, market participants have no difficulty in estimating the extent of securitization recourse. For example, it is possible that for some banks every market participant agrees that the bank's securitization transactions are true sales of assets or secured borrowings. This possibility adds tension to our hypothesis.

[8] From a conceptual point of view, there is an important distinction between intrinsic risk and information uncertainty. Following recent literature (e.g., Anderson et al. 2009), we adopt the position that an event is *risky* if its outcome is unknown, but the distribution of its potential outcomes is known; an event is *uncertain* if its outcome is unknown and the distribution of its outcomes is also unknown. To illustrate this difference in our context we offer an example. For a bank that securitizes assets, market participants know there is a possibility of future recourse. To compute the expected value of future support, market participants take into account a set of possible future economic outcomes and attach a point probability estimate to each outcome. This scenario assumes that market participants are able to develop a *point* probability for each possible outcome. In this case, the market participants face risk with respect to securitization recourse. However, in some circumstances, market participants do not have accurate and complete information to assign a point probability (i.e., the distribution of probabilities for each outcome cannot be reduced to a single point). In this case, market participants face information uncertainty with respect to securitization recourse. Our concept of information uncertainty is similar to the notion of Knightian uncertainty, long established in the economic literature (e.g., Keynes 1937; Knight 1921).

Following prior studies (Chordia et al. 2001; Flannery et al. 2004), we use both the consolidated trades and the consolidated quote files from the TAQ database to compute the effective bid-ask spreads. We use CRSP information to compute the share turnover and return volatility measures, and the I/B/E/S summary file to compute analyst forecast dispersion and analyst following measures. After eliminating observations with missing values for the necessary control variables used in each of our empirical models, we obtain a sample of 871 and 687 bank-quarter securitization observations for the spread and dispersion models, respectively.

Table 1 presents descriptive statistics for the more comprehensive sample used in our bid-ask spread analysis (n = 871 bank-quarter observations). For the banks in our sample, net loans represent 60.96 percent of total assets, quarterly net income is 0.66 percent of total assets, the liability-to-equity ratio averages about 11.07, and the log value of total assets is 9.88. Bank return volatility has a mean of 0.015 and the average price per share is $34.78. Close to a third of our sample banks (31.34 percent) are listed on the NASDAQ, and the rest are listed on the NYSE or on the AMEX. In general, the descriptive statistics for our sample are comparable to those reported by Flannery et al. (2004) and Chen et al. (2008). For the sample used in the forecast dispersion analysis (n = 687), the untabulated mean (median) value of forecast dispersion is 0.021 (0.013). All other descriptive statistics are similar to the statistics presented in Table 1.

V. RESEARCH DESIGN AND FINDINGS

Information Uncertainty Measures

To test our hypothesis, we use two proxies for bank information uncertainty: bid-ask spreads and analyst forecast dispersion. The bid-ask spread is commonly used in the literature as a measure of information asymmetry (e.g., Flannery et al. 2004; Leuz and Verrecchia 2000).[11] When

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[9] A bank holding company is any firm that controls a bank, where control is defined as holding more than 25 percent of a bank's equity or having the ability to elect more than two directors. Our sample of bank holding companies does not include investment banks.

[10] We include in our sample all the available quarters for the securitizing banks, even if a bank does not securitize in each quarter during our sample period. To test the sensitivity of our results to this sample selection choice, we run two types of robustness tests. We rerun our analyses using: (1) only those bank-quarter observations with positive securitization volume and (2) only banks that have at least six quarters with positive securitization volume during our sample period (following Chen et al. 2008). Our results do not change.

[11] We note that, besides an adverse-selection component, the bid-ask spreads also include an order-processing component and an inventory-holding component. In this study, we are interested in the adverse-selection component of the spreads. However, the empirical decomposition of the bid-ask spreads into the three distinct components is not very reliable

TABLE 1
Distributional Characteristics of the Variables Used in the Multivariate Analysis
(n = 871)

Variable	Mean	Median	Q1	Q3	Std. Dev.
SPREAD	0.0056	0.0023	0.0013	0.0059	0.0080
NETLNS	0.6096	0.6544	0.5400	0.7136	0.1569
SIZE	9.8841	10.3482	8.5890	11.4642	2.1311
PROFIT	0.0066	0.0062	0.0032	0.0099	0.0052
MVLEV	11.0740	10.3285	9.3194	12.2134	3.0155
STDRET	0.0151	0.0133	0.0104	0.0181	0.0070
PRICE	34.7803	30.5400	23.1700	42.3200	17.5294
NASDAQ	0.3134	0.0000	0.0000	1.0000	0.4642
TURNOVER	0.1118	0.0881	0.0586	0.1387	0.0868
FOLLOW	2.1087	2.6391	1.3863	2.9957	1.1484
DERIVATIVE	1.3407	0.1378	0.0234	0.6977	3.4736
CHGOFF_ONBS	0.0034	0.0022	0.0009	0.0043	0.0038
NPL_ONBS	0.0112	0.0100	0.0060	0.0144	0.0074
GAP	0.1900	0.1864	0.0857	0.2750	0.1227

Table 1 provides descriptive statistics for the variables used in our multivariate analysis for the bid-ask spread models. These descriptive statistics are computed using a sample of securitizing banks. See the Appendix for variable definitions.

information asymmetry among market participants is high, informed traders can exploit their informational advantage at the expense of uninformed traders. The market-makers realize they face an adverse-selection problem and increase the bid-ask spread to protect against expected losses from trading with more informed investors. This argument suggests a positive association between the degree of information asymmetry and bid-ask spreads.

In addition to bid-ask spreads, we also use analyst forecast dispersion as a measure of information uncertainty. Our choice of analyst-related information uncertainty measures is consistent with prior literature (e.g., Leuz 2003; Krishnaswami and Subramaniam 1999) that interprets disagreement among analysts as an indication of the lack of available information about the firm and uses the dispersion of analysts' earnings forecasts as measures of firm-level information asymmetry and information quality. In the context of our study, analyst forecast dispersion is a relevant information uncertainty proxy because analysts focus on firm profitability and securitization directly affects profitability. For the banks in our sample, securitization is an important source of

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(Flannery et al. 2004; Van Ness et al. 2001). Prior literature (Van Ness et al. 2001) examining the economic validity of the various models used to decompose bid-ask spreads finds that different models lead to widely varying magnitudes of the estimated adverse-selection component. Van Ness et al. (2001, 96) also find that the adverse-selection estimate is poorly correlated with alternate indicators of asymmetric information and conclude that some of the decomposition models represent merely "noisy transformations of the spread." Therefore, in this study, we chose to use the actual (not decomposed) bid-ask spreads and to include in our multivariate analyses control variables (e.g., the stock exchange listing and a firm's idiosyncratic risk) that proxy for order-processing and inventory-holding costs.

earnings.[12] In addition, financial analysts consistently rate securitization-related disclosure as high in importance, yet assign low ratings for securitization disclosure quality (CFA Institute 2008).[13]

Recourse Uncertainty Measures

Ideally, we would use a direct measure of uncertainty that various market participants face with respect to the true extent of recourse obligations. However, such a measure is not directly observable. Therefore, in our empirical analysis we select several measures that we expect to be correlated with the uncertainty about the extent of recourse. These measures are intended to capture uncertainly about the need for future support for troubled securitization transactions and about banks' willingness to provide such support.

First, we select a bank's securitization volume (*ABS*) and securitization income (*SECINC*) because market participants likely find it more difficult to assess the anticipated need for future support for banks with a larger securitization volume/income. In the presence of higher securitization levels, market participants face greater information uncertainty with respect to the availability and the extent of suitable reinvestment opportunities for the larger amounts of securitization-generated cash. Typically, banks recycle securitization proceeds by using the cash to generate or purchase the same types of assets that were originally securitized (e.g., mortgage loans) and then securitize the new assets as well. Prior studies (Dell'Ariccia et al. 2008; Keys et al. 2009) find that, in an attempt to generate enough loans to maintain a high securitization volume and securitization fees, banks lower their lending standards. In this context, opportunities to reinvest securitization proceeds and lending standards are important in assessing the quality of securitized assets and thus, the need for future support. However, bank outsiders face increased information uncertainty in the presence of larger securitization volume/income because investment opportunities and lending standards are not directly observable to outsiders. In addition, banks with higher securitization volume/income are typically more likely to engage in more complex and more heterogeneous securitization transactions and have a wider range of possible recourse obligations, leaving information users exposed to greater information uncertainty.

Second, we use two proxies for the quality of the securitized assets to capture recourse uncertainty: (1) the proportion of nonperforming loans associated with securitized loans (*NPL_SEC*) and (2) the charge-offs associated with securitized loans (*CHOFF_SEC*). Market participants likely find it more difficult to assess banks' willingness and methods to offer future support to their troubled securitization transactions in the presence of a higher anticipated need for support. In deciding whether to offer voluntary support, banks trade off the reputation benefits for the costs of such support (Vermilyea et al. 2008). When the expected levels of necessary future support are higher, banks get closer to the point where the costs outweigh the benefits. Because outside information users cannot observe the break-even point, they are likely to find it more difficult to assess banks' willingness to offer implicit recourse when the expectations about necessary future support are high. In addition, when the need for the future support is high, banks could be forced to take more types of recourse measures to support their securitization transactions.[14] Consequently, information users are likely to be exposed to greater information uncertainty in the presence of a greater need for support, if a greater need for recourse is associated with a heterogeneous mix of recourse actions.

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[12] Securitizing banks' reported earnings depend on the volume and profitability of their securitizations because, at the date a securitization transaction takes place, sale accounting requires banks to derecognize the securitized assets, record the cash proceeds, and record a gain or a loss from securitization. Transaction and servicing fees generated through securitization are also important components of total earnings. In our sample, the proportion of income coming from securitization activities (i.e., securitization gains and losses and servicing fees scaled by total net income) represents, on average, about 22 percent of total bank income (median value about 4 percent).

[13] The importance of securitization activities from analysts' point of view is also supported by anecdotal evidence. For the banks in our sample, we find anecdotal evidence in conference call transcripts that, in some cases, analysts ask clarifying questions about the magnitude of the expected securitization gains mentioned by bank managers, or managers provide additional details about the magnitude of expected securitization recourse.

Also, we use the volume of asset-backed securities retained by securitizing banks on their balance sheets (i.e., retained interests, *RET_INT*) to capture uncertainty about explicit recourse. For securitizing banks, information users can only observe the reported levels of the retained interests, but they do not have detailed information about the retained interests' level of subordination, credit enhancement features, and other details of various heterogeneous securitization transactions necessary to infer the expected future support. The argument that information users can only observe aggregate reported levels of securities holds for other types of on-balance sheet securities as well. However, compared to other securities, retained interests have several features that make them subject to greater information uncertainly. The value of the retained interests is closely intertwined with the value of the pool of securitized assets transferred to the SPEs, representing a highly concentrated risk position. That is, when the pool of transferred assets suffers losses, the retained interests bear significantly more than their proportional share of the losses because of their first-loss position. In addition, retained interests are very illiquid because there is no active market for these securities. Therefore, their valuation depends heavily on managerial assumptions, leaving outside investors exposed to high levels of information asymmetry (Dechow and Shakespeare 2009; Dechow et al. 2010). High levels of retained interests also imply a higher upper limit of possible explicit recourse, which, in turn, likely leads to more uncertainly about the extent of risk transfer achieved through securitization.

It is possible that each of the five recourse uncertainty proxies captures only specific aspects of the underlying concept of interest (i.e., the uncertainty about the extent of risk transfer) and, therefore, includes measurement errors. To mitigate this concern, we use a principal component analysis to extract the relevant information in these measures and compute a composite recourse uncertainty factor.

The Impact of the Difficulty in Estimating Recourse on Bank Information Uncertainty

To test our hypothesis that the difficulty in estimating recourse obligations is positively associated with bank information uncertainty, we regress the two information uncertainty proxies (bid-ask spreads and analyst forecast dispersion) on our recourse uncertainty factor and control variables. Our regressions are specified as follows (firm and quarter subscripts omitted):

$$\begin{aligned} UNCERTAINTY = {} & a_0 + a_1 RECOURSE + a_2 NETLNS + a_3 PROFIT + a_4 MVLEV + a_5 SIZE \\ & + a_6 STDRET + (a_7 PRICE + a_8 NASDAQ + a_9 TURNOVER) + a_{10} FOLLOW \\ & + a_{11} DERIVATIVE + a_{12} CHGOFF\_ONBS + a_{13} NPL\_ONBS + a_{14} GAP + e. \end{aligned} \tag{1}$$

UNCERTAINTY is defined, in turn, as either the bid-ask spread (*SPREAD*) or analyst forecast dispersion (*DISPERSION*). Our main variable of interest in Equation (1) is *RECOURSE*. This variable is the first factor from a principal component analysis using *ABS*, *SECINC*, *NPL_SEC*, *CHOFF_SEC*, and *RET_INT*. If our hypothesis holds, then we expect a positive coefficient on the recourse factor.

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[14] These various types of actions can have different future implications for the securitizing bank. For example, to support its securitization structures, a bank can sell assets to a securitization trust (or other SPE) at a discount from the price specified in the securitization documents, which is typically par value. At the same time, the bank can fund credit enhancements beyond contractual requirements. Arguably, the costs of the former type of implicit recourse measure are easier to assess than the costs of the latter type.

In Equation (1), the variables included in parentheses are relevant only for the model in which the bid-ask spread is the dependent variable. Prior research finds that firm size and analyst following are associated with information uncertainty. Therefore, we include these control variables in our empirical models. We expect firm size and analyst following (*SIZE* and *FOLLOW*) to be negatively associated with our measures of information uncertainty because the greater information availability for larger firms and firms with larger analyst following can reduce information uncertainty. Following prior literature, we use bank stock price, share turnover, and an indicator variable for a firm's stock exchange listing in our spread regression. The indicator variable for the exchange listing (*NASDAQ*) is expected to be positively associated with spreads, since prior research finds that firms listed on the NASDAQ stock exchange have larger bid-ask spreads as compared to the NYSE and AMEX firms. Price is expected to have a negative coefficient in the spread regressions. We also include share turnover (*TURNOVER*) as a control variable in the bid-ask spread model and predict a negative coefficient on turnover, because prior literature finds that spreads decrease with trading volume.

To control for differences in bank asset composition, we include the proportion of on-balance sheet net loans to total assets (*NETLNS*), as loans are by far the largest asset on our sample firms' balance sheet. Because prior studies (e.g., Flannery et al. 2004) find mixed results with respect to whether more loans increase risk, we do not have directional predictions for this variable. In addition, we include the ratio of net income to total assets (*PROFIT*) to capture the effect of banks' profitability on information uncertainty.

One caveat of using proxies associated with the difficulty in estimating recourse is that our recourse uncertainty factor might only capture the securitization recourse level. Because the recourse level is a determinant of a bank's intrinsic risk, our recourse uncertainty measure might capture the effect of securitization on bank intrinsic risk and not the effect on bank information uncertainty. Therefore, in Equation (1), we use controls frequently used in prior literature as proxies for firm risk, such as the standard deviation of returns (*STDRET*) and firm leverage (*MVLEV*).[15] In addition, we include several proxies for bank risk that are specific to the banking industry (*DERIVATIVE*, *CHGOFF_ONBS*, *NPL_ONBS*, and *GAP*). We control for banks' derivative use to account for the possibility that banks make the decision of how much recourse to offer in association with their securitized assets in the broader context of an overall risk-management strategy. Prior research (e.g., Minton et al. 2009) finds that banks are more likely to buy derivative protection if they are engaged in asset securitization. Given the level of complexity and illiquidity of some derivative transactions, it is possible that the use of derivatives also leads to increased bank information uncertainty and thus, we expect a positive coefficient on *DERIVATIVE*. The next two measures (*CHGOFF_ONBS* and *NPL_ONBS*) are intended to capture the riskiness of on-balance sheet loans. If market participants face greater uncertainty in assessing banks that have exposure to riskier on-balance sheet loans, then we expect a positive association between the *CHGOFF_ONBS* and *NPL_ONBS* variables and our measures of information uncertainty. The *GAP* variable captures the interest rate risk stemming from the possible mismatching between assets and liabilities, which will be repriced in the short-run. We expect positive coefficients on all of these risk variables. Note that, to some extent, the bank size and the on-balance sheet net loans also control for differences in risk profile among securitizing banks.

Panel A of Table 2 presents the distributional characteristics of the five measures of recourse uncertainty and the first common factor based on these measures for our sample of securitizing

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[15] Although, prior information asymmetry literature does not traditionally include beta (i.e., a systematic risk measure) as a control variable in bid-ask spreads and forecast dispersion regressions, in untabulated analysis we add firm beta as an additional risk measure. Our results are robust to the inclusion of this control variable.

TABLE 2

Descriptive Statistics for Recourse Uncertainty Variables
(n = 871)

Panel A: Distributional Characteristics of the Recourse Uncertainty Variables

Variables	Mean	Median	Q1	Q3	Std. Dev.
ABS	0.1162	0.0141	0.0000	0.0817	0.2573
NPL_SEC	0.0045	0.0001	0.0000	0.0025	0.0118
CHGOFF_SEC	0.0004	0.0000	0.0000	0.0001	0.0010
SECINC	0.2196	0.0368	0.0100	0.1830	0.4889
RET_INT	0.0010	0.0000	0.0000	0.0007	0.0024
RECOURSE	0.0000	−0.4340	−0.5295	0.1211	1.0000

Panel B: Correlations of the Recourse Uncertainty Variables

Variables	*ABS*	*NPL_SEC*	*CHGOFF_SEC*	*SECINC*	*RET_INT*	*RECOURSE*
ABS	1.00	0.95***	0.42***	0.59***	0.52***	0.92***
NPL_SEC	0.91***	1.00	0.47***	0.59***	0.62***	0.92***
CHGOFF_SEC	0.64***	0.73***	1.00	0.70***	0.73***	0.65***
SECINC	0.54***	0.62***	0.51***	1.00	0.63***	0.75***
RET_INT	0.64***	0.70***	0.82***	0.49***	1.00	0.66***
RECOURSE	0.83***	0.87***	0.70***	0.83***	0.67***	1.00

Panel C: Weights (Scoring Coefficients) on the Five Recourse Uncertainty Measures in the First Common Factor

	ABS	*NPL_SEC*	*CHGOFF_SEC*	*SECINC*	*RET_INT*
Weight	0.2174	0.3956	0.2233	0.1450	0.1650

*** Denotes statistical significance at 1 percent based on two-tailed tests.

Table 2 provides descriptive statistics for the recourse uncertainty variables. These descriptive statistics are computed using a sample of securitizing banks.

Panel A reports the descriptive statistics.

Panel B reports correlations between various recourse uncertainty variables. Pearson (Spearman) correlations are presented above (below) the diagonal.

Panel C presents the weight (standardized scoring coefficients) on each of the five measures in a linear combination of measures and the weights that yield the first-factor scores from our principal component analysis.

See the Appendix for variable definitions.

banks. On average, total securitized assets, nonperforming securitized loans, total charge-offs associated with securitized loans, and total retained interests account for 11.62 percent, 0.45 percent, 0.04 percent, and 0.10 percent of total assets, respectively. Securitization income (including service fees) accounts for 21.96 percent of net income of these banks. Panel B of Table 2 presents Pearson and Spearman correlations for the recourse variables. The correlations between these proxies for implicit recourse and retained interests are high, ranging from 0.42 (Pearson correlation between *ABS* and *CHCOFF_SEC*) to 0.95 (Pearson correlation between *ABS* and *NPL_SEC*). These high correlations are reasonable, given our argument that all five measures represent the same underlying concept, i.e., the extent of recourse related to securitized assets. However, the results in Table 2 show that, although highly correlated, these five variables do not perfectly overlap and likely capture different aspects of the uncertainty of risk transfer. Panel C of Table 2 presents the weights (i.e., the standardized scoring coefficients) on the five recourse uncertainty measures used to compute the first common factor from our principal component analysis. These weights indicate that all five measures contribute substantially to the computation of our recourse uncertainty factor. We retain only the first common factor in the principal component analysis, since this is the only factor with an eigenvalue greater than 1. This first factor retains about 70 percent of the total variance of the original data.

Table 3 presents the results of estimating Equation (1) using a sample of securitizing banks. Model 1 (Model 2) uses the effective bid-ask spread (analyst forecast dispersion) as our proxy for information uncertainty. The findings in Table 3 support our hypothesis that the level of difficulty in estimating securitization recourse is positively associated with bank-level information uncertainty. Consistent with our expectations, the recourse uncertainty factor has statistically significant, positive coefficients in both the spread (t-statistic = 2.10; one-tailed p-value = 0.05) and the analyst forecast dispersion regressions (t-statistic = 1.75; one-tailed p-value = 0.05). All the control variables are either consistent with our predictions or insignificant, with one exception. While we predict a positive coefficient on the charge-offs on balance sheet loans variable (*CHGOFF_ONBS*), we find a negative and marginally significant (t-statistic = −1.74; two-tailed p-value = 0.10) coefficient in the bid-ask spread model.

The Impact of the Information Environment Quality on the Relation between Recourse and Information Uncertainty

Given our finding that, on average, the difficulty in assessing the extent of securitization recourse for securitizing banks is positively associated with bank information uncertainty, we next investigate whether this effect is mitigated for banks that operate in a higher quality information environment. To test this hypothesis, we run the following regression (firm and quarter subscripts omitted):

$$\begin{aligned} UNCERTAINTY = {} & a_0 + a_1 RECOURSE + a_2 RECOURSE * QUALITY\_INFO\_ENV \\ & + a_3 QUALITY\_INFO\_ENV + a_4 NETLNS + a_5 PROFIT + a_6 MVLEV \\ & + a_7 STDRET \; (+ a_8 PRICE + a_9 NASDAQ + a_{10} TURNOVER) \\ & + a_{11} DERIVATIVE + a_{12} CHGOFF\_ONBS + a_{13} NPL\_ONBS + a_{14} GAP + e. \end{aligned} \tag{2}$$

The *QUALITY_INFO_ENV* variable is defined, in turn, as either analyst following (*FOLLOW*), or the level of institutional investor holdings (*INST*), or size (*SIZE*). If our hypothesis holds, then we expect the coefficient on the interaction term between the recourse uncertainty

TABLE 3
The Impact of Recourse Uncertainty on Banks' Bid-Ask Spreads and Analyst Forecast Dispersion

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	**t-statistic**	**Coefficient**	**t-statistic**
Intercept	?	0.0255	(5.19)***	0.0159	(0.75)
RECOURSE	+	0.0013	(2.10)**	0.0069	(1.75)**
NETLNS	?	−0.0044	(−1.27)	0.0011	(0.09)
PROFIT	?	0.0294	(0.40)	0.0029	(0.01)
MVLEV	?	0.0001	(0.85)	0.0018	(2.95)***
SIZE	−	−0.0017	(−3.31)***	−0.0007	(−0.43)
STDRET	+	0.1656	(1.93)**	0.4045	(0.95)
PRICE	−	−0.0001	(−1.26)	NA	NA
NASDAQ	+	0.0016	(1.43)*	NA	NA
TURNOVER	−	−0.0124	(−2.13)**	NA	NA
FOLLOW	−	−0.0019	(−2.07)**	−0.0056	(−2.19)**
DERIVATIVE	+	0.0003	(2.65)***	0.0006	(3.10)***
CHGOFF_ONBS	+	−0.2007	(−1.74)*	−0.5754	(−0.73)
NPL_ONBS	+	0.0900	(1.12)	0.0358	(0.15)
GAP	+	0.0046	(1.80)**	0.0035	(0.25)
Adj-R^2		55.09%		23.27%	
n		871		687	

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*, **, *** Denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on one-tailed tests if the coefficient has a predicted sign, and two-tailed tests otherwise.

Table 3 presents the results from the regressions of banks' bid-ask spreads and analyst forecast dispersion on the recourse uncertainty factor and control variables using a sample of securitizing banks. Standard errors are cluster-adjusted by firm and quarter following Petersen (2009) and Gow et al. (2010).

See the Appendix for variable definitions.

factor and the quality of the information environment to be negative (i.e., $a_2 < 0$) and the coefficient on the recourse uncertainty factor to be positive (i.e., $a_1 > 0$).

We select analyst following as a proxy for the quality of the information environment because prior research finds that when analyst following is large, greater competition among analysts leads to more timely price discovery (e.g., Brennan et al. 1993; Brennan and Subrahmanyam 1996) and lower information asymmetries in markets (e.g., Hong et al. 2000; Barth and Hutton 2004; Ellul and Panayides 2008). A separate stream of literature (Lang and Lundholm 1996) finds that analysts are more likely to cover firms with higher quality disclosure policies. We use the level of institutional investor holdings as a second proxy for the quality of the information environment because institutional investors have a higher level of expertise in processing relevant information. Such expertise is particularly important in our setting because the underlying securitization transactions are very complex and thus, by necessity, the securitization accounting and disclosures are technically complex. In addition, we choose bank size as our third measure of information environment quality because larger banks are likely to attract more press coverage and information users' scrutiny, leading to a more timely and comprehensive information discovery process.[16,17]

The findings presented in Table 4 (Panels A, B, and C) support our hypothesis. The coefficients on the interaction term between the recourse uncertainty factor and the information environment quality are negative and statistically significant for all three measures of information environment quality, for both the bid-ask spread and the forecast dispersion regressions. In addition, supporting our hypothesis, the coefficients on the recourse uncertainty factor are positive and statistically significant. All control variables are either consistent with our predictions or statistically insignificant at conventional significance levels.

Our findings show that, for securitizing banks, there is a positive association between the difficulty in estimating recourse and bank information uncertainty, even after including in our regressions control measures commonly used in the literature as proxies for intrinsic risk. However, to the extent that these typical risk controls measure bank intrinsic risk with error, it could be that our recourse uncertainty factor captures the impact of securitization on bank intrinsic risk, while we spuriously attribute the effect to information uncertainty. The findings presented in Table 4 help alleviate this concern. On the one hand, if our recourse uncertainty factor captures the information effect of securitization, then we expect the impact of recourse measures on bank information uncertainty to be mitigated for banks that operate in a high-quality information environment. On the other hand, if the factor captures only the impact of recourse on bank risk, then we would not expect the relation between recourse and information uncertainty to vary systematically with the quality of the information environment. The results in Table 4 offer support for the former expectation.

Following Fernandes and Ferreira (2008), to further rule out the alternative explanation that our recourse uncertainty factor captures only the impact of securitization on bank intrinsic risk and not the impact on information uncertainty, we also investigate whether our quality-of-information-environment results are robust to controlling for the impact of intrinsic risk on the relation between our recourse uncertainty measure and bank information uncertainty. Specifically we estimate the following equation (firm and quarter subscripts omitted):

$$\begin{aligned} UNCERTAINTY = {} & a_0 + a_1 RECOURSE + a_2 RECOURSE * QUALITY\_INFO\_ENV \\ & + a_3 RECOURSE * STDRET + a_4 QUALITY\_INFO\_ENV + a_5 NETLNS \\ & + a_6 PROFIT + a_7 MVLEV + a_8 STDRET \; (+ a_9 PRICE + a_{10} NASDAQ \\ & + a_{11} TURNOVER) + a_{12} DERIVATIVE + a_{13} CHGOFF\_ONBS \\ & + a_{14} NPL\_ONBS + a_{15} GAP + e. \end{aligned} \tag{3}$$

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[16] Note that there are two types of information that affect a bank's information environment and can help mitigate the uncertainty about the extent of recourse: (1) securitization transaction-specific information about the quality of securitized assets and the implicit and explicit guaranties of support offered by securitizing banks; and (2) general bank information about investment opportunities, lending standards, alternative sources of financing, and bank reputation concerns, etc. Ideally, we would assess the quality of a bank information environment by investigating both these types of information. In untabulated analysis, we hand-collect securitization transaction disclosures for our sample banks from the footnotes to the financial statements included in the 10-K filings. Consistent with complaints by investors and financial analysts, we find that the securitization-related footnote disclosure is relatively standardized, making it very difficult to capture cross-sectional variation in the quality of securitization footnote disclosure for our sample firms. Therefore, in our empirical analyses we focus on bank-level information quality aspects.

[17] In a sensitivity test, we use principal component analysis to develop an information environment quality factor based on size, institutional holding, and analyst following. We use this information quality factor to replace each individual information environment measure and rerun the analyses in Tables 4 and 5. We find that our results are robust to this alternative specification.

TABLE 4

The Impact of Information Environment Quality on the Relation between Recourse Uncertainty and Banks' Bid-Ask Spreads and Analyst Forecast Dispersion

Panel A: Using Analyst Following (*FOLLOW*) as a Measure of the Quality of the Information Environment

		Bid-Ask Spread		Forecast Dispersion	
	Predicted Sign	Coefficient	t-statistic	Coefficient	t-statistic
Intercept	?	0.0267	(5.18)***	0.0159	(0.70)
RECOURSE	+	0.0033	(2.94)***	0.0340	(3.52)***
*RECOURSE * FOLLOW*	−	−0.0010	(−1.82)**	−0.0104	(−4.02)***
NETLNS	?	−0.0052	(−1.44)	−0.0021	(−0.16)
PROFIT	?	−0.0716	(−0.90)	−0.5845	(−1.84)*
MVLEV	?	0.0001	(0.64)	0.0016	(2.37)**
SIZE	−	−0.0016	(−3.29)***	0.0001	(0.03)
STDRET	+	0.1463	(1.73)**	0.3036	(0.80)
PRICE	−	−0.0001	(−1.31)	NA	NA
NASDAQ	+	0.0018	(1.57)*	NA	NA
TURNOVER	−	−0.0160	(−2.92)***	NA	NA
FOLLOW	−	−0.0021	(−2.35)***	−0.0055	(−1.91)**
DERIVATIVE	+	0.0002	(2.10)**	0.0003	(1.09)
CHGOFF_ONBS	+	−0.0794	(−0.58)	0.1491	(0.28)
NPL_ONBS	+	0.1053	(1.28)	0.0921	(0.46)
GAP	+	0.0042	(1.71)*	−0.0014	(−0.11)
Adj-R^2		56.70%		28.35%	
n		871		687	

Panel B: Using Institutional Holding (*INST*) as a Measure of the Quality of the Information Environment

		Bid-Ask Spread		Forecast Dispersion	
	Predicted Sign	Coefficient	t-statistic	Coefficient	t-statistic
Intercept	?	0.0250	(5.06)***	0.0095	(0.34)
RECOURSE	+	0.0032	(3.76)***	0.0210	(2.16)**

(continued on next page)

Panel B: Using Institutional Holding (*INST*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	t-statistic	Coefficient	t-statistic
RECOURSE ∗ *INST*	−	−0.0039	(−2.37)∗∗∗	−0.0262	(−1.97)∗∗
INST	?	−0.0034	(−1.08)	0.0210	(1.13)
NETLNS	?	−0.0038	(−1.06)	0.0077	(0.47)
PROFIT	?	−0.0385	(−0.65)	−0.2840	(−0.77)
MVLEV	?	0.0000	(−0.14)	0.0020	(2.41)∗∗
SIZE	−	−0.0013	(−2.83)∗∗∗	−0.0007	(−0.36)
STDRET	+	0.1714	(2.35)∗∗∗	0.3549	(0.90)
PRICE	−	−0.0001	(−1.35)∗	NA	NA
NASDAQ	+	0.0013	(1.25)	NA	NA
TURNOVER	−	−0.0132	(−2.38)∗∗∗	NA	NA
FOLLOW	−	−0.0015	(−1.39)∗	−0.0094	(−2.76)∗∗∗
DERIVATIVE	+	0.0002	(2.31)∗∗	0.0005	(1.83)∗∗
CHGOFF_ONBS	+	−0.1352	(−1.44)	−0.1127	(−0.18)
NPL_ONBS	+	0.0255	(0.47)	0.2079	(0.97)
GAP	+	0.0019	(0.81)	0.0043	(0.34)
Adj-R^2		59.01%		27.11%	
n		871		687	

Panel C: Using Size (*SIZE*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	t-statistic	Coefficient	t-statistic
Intercept	?	0.0262	(5.07)∗∗∗	0.0159	(0.76)
RECOURSE	+	0.0049	(1.89)∗∗	0.0453	(2.14)∗∗
RECOURSE ∗ *SIZE*	−	−0.0004	(−1.40)∗	−0.0038	(−1.94)∗∗
NETLNS	?	−0.0045	(−1.31)	0.0024	(0.18)
PROFIT	?	−0.0256	(−0.36)	−0.2637	(−0.74)
MVLEV	?	0.0001	(0.84)	0.0018	(2.81)∗∗∗
SIZE	−	−0.0017	(−3.21)∗∗∗	−0.0010	(−0.58)
STDRET	+	0.1499	(1.74)∗∗	0.3372	(0.86)

(continued on next page)

Panel C: Using Size (*SIZE*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	**t-statistic**	**Coefficient**	**t-statistic**
PRICE	−	<−0.0001	(−1.35)*	NA	NA
NASDAQ	+	0.0017	(1.54)*	NA	NA
TURNOVER	−	−0.0150	(−2.87)***	NA	NA
FOLLOW	−	−0.0019	(−2.05)**	−0.0041	(−1.37)*
DERIVATIVE	+	0.0003	(2.62)***	0.0006	(2.62)***
CHGOFF_ONBS	+	−0.1238	(−0.93)	−0.2052	(−0.30)
NPL_ONBS	+	0.0931	(1.17)	0.0294	(0.14)
GAP	+	0.0044	(1.77)**	0.0006	(0.04)
Adj-R^2		55.89%		24.56%	
n		871		687	

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*, **, *** Denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on one-tailed tests if the coefficient has a predicted sign, and two-tailed tests otherwise.

Table 4 presents the results from the regressions of banks' bid-ask spreads and analyst forecast dispersion on the recourse uncertainty factor, the recourse uncertainty factor interacted with information environment quality proxies, and control variables. The analyses in Panels A, B, and C use the number of analysts following, the percentage of institutional holding, and the firm size, respectively, to measure the quality of the information environment. Standard errors are cluster-adjusted by firm and quarter following Petersen (2009) and Gow et al. (2010)

See the Appendix for variable definitions.

All variables are defined as before. Compared with Equation (2), Equation (3) includes an additional interaction term between recourse uncertainty and intrinsic risk.

If the factor captures the information effect of securitization, then we expect the impact of recourse measures on bank information uncertainty to be mitigated for banks that operate in a higher quality information environment even after adding an interaction term between recourse and intrinsic risk (as measured by the standard deviation of returns). In other words, we expect the coefficient on the interaction term between the recourse uncertainty factor and the quality of the information environment to be negative (i.e., $a_2 < 0$) and the coefficient on the recourse uncertainty factor to be positive (i.e., $a_1 > 0$) even after controlling for $RECOURSE * STDRET$.

As shown in Table 5 (Panels A, B, and C), consistent with our prediction, the coefficients on the interaction term between the recourse uncertainty factor and the information environment quality are negative and statistically significant for all three measures of information environment quality.[18] In addition, the coefficients on the recourse uncertainty factor remain positive and statistically significant in all of these alternative specifications. However, the coefficients on the interaction term $RECOURSE * STDRET$ are not statistically significant at conventional levels for any of our specifications, indicating that the relation between the factor and bank information uncertainty does not systematically vary with the level of bank intrinsic risk (as measured by the standard deviation of stock returns). These findings further alleviate the concern that our findings capture only the impact of securitization recourse on bank risk.

The Impact of Securitization on Information Uncertainty

So far, our findings indicate that the difficulty in estimating recourse has a significant impact on bank information uncertainty for a sample of securitizing banks and this effect is mitigated for banks that operate in a higher quality information environment. An interesting, related question is whether securitization leaves banks that engage in this type of financing transactions more or less exposed to information uncertainty as compared to banks that do not securitize assets. Since securitization transactions can impact information uncertainty through several mechanisms, it is not clear *ex ante* whether securitizing banks face higher or lower levels of information uncertainty compared to banks that do not securitize assets. First, securitizing banks are exposed to uncertainty about whether the risks associated with the securitized assets have been transferred from the originating bank to investors in asset-backed securities. Second, compared to non-securitizing banks, banks that undertake securitization transactions are exposed to uncertainty regarding the effect of reinvesting the considerable amounts of securitization proceeds. Market participants face uncertainty about the availability of suitable reinvestment opportunities for the cash proceeds. These two mechanisms would suggest higher information uncertainty for securitizing compared to non-securitizing banks. Third, in the securitization process, banks have to satisfy additional disclosure requirements for the transferred assets.[19] By providing the additional required disclosure, securitization can lead to increased transparency with respect to not only the securitized assets, but also the securitizing firms in general (Foley et al. 1999; Berger and Udell 1995; Cantor and Rouyer 2000). Fourth, it is possible that the difficulty in estimating recourse is so great that

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[18] When we control for bank intrinsic risk using beta as opposed to the standard deviation of returns, we obtain similar results.

[19] For example, a prospectus issued by an SPE to register asset-backed securities with the SEC can include information about initial principal balances for various classes of securities issued, pass-through rates, principal type, interest type, initial credit ratings, etc. It can also include a discussion of various risk factors and information about servicers and servicing fees, such as a description of the different components of the servicing fees, various types of servicer expenses, supplemental servicing fee schedules, etc. Because in many securitizations, the securitizing bank also retains the role of servicer, the prospectus information about the servicer is directly relevant to bank outsiders in assessing the securitizing bank's business model and sources of revenue.

TABLE 5

The Impact of Information Environment Quality and Risk on the Relation between Recourse Uncertainty and Banks' Bid-Ask Spreads and Analyst Forecast Dispersion

Panel A: Using Analyst Following (*FOLLOW*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	t-statistic	Coefficient	t-statistic
Intercept	?	0.0268	(5.15)***	0.0157	(0.70)
RECOURSE	+	0.0038	(2.30)**	0.0328	(3.65)***
RECOURSE * *FOLLOW*	−	−0.0010	(−1.81)**	−0.0104	(−4.07)***
RECOURSE * *STDRET*	?	−0.0167	(−0.53)	0.0520	(0.68)
NETLNS	?	−0.0052	(−1.43)	−0.0020	(−0.15)
PROFIT	?	−0.0727	(−0.91)	−0.5854	(−1.84)*
MVLEV	?	0.0001	(0.54)	0.0016	(2.38)**
SIZE	−	−0.0016	(−3.31)***	0.0001	(0.04)
STDRET	+	0.1511	(1.74)**	0.2911	(0.78)
PRICE	−	−0.0001	(−1.29)	NA	NA
NASDAQ	+	0.0018	(1.58)*	NA	NA
TURNOVER	−	−0.0158	(−3.01)***	NA	NA
FOLLOW	−	−0.0021	(−2.35)**	−0.0055	(−1.92)**
DERIVATIVE	+	0.0002	(2.09)**	0.0003	(1.11)
CHGOFF_ONBS	+	−0.0817	(−0.60)	0.1559	(0.29)
NPL_ONBS	+	0.1060	(1.28)	0.0906	(0.45)
GAP	+	0.0041	(1.69)**	−0.0015	(−0.11)
Adj-R^2		56.68%		28.28%	
n		871		687	

Panel B: Using Institutional Holding (*INST*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	t-statistic	Coefficient	t-statistic
Intercept	?	0.0251	(5.08)***	0.0091	(0.33)

(continued on next page)

Panel B: Using Institutional Holding (*INST*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	t-statistic	Coefficient	t-statistic
RECOURSE	+	0.0034	(4.77)***	0.0194	(2.29)**
*RECOURSE * INST*	–	−0.0040	(−2.57)***	−0.0262	(−1.97)**
*RECOURSE * STDRET*	?	−0.0069	(−0.38)	0.0759	(0.71)
INST	?	−0.0034	(−1.11)	0.0213	(1.14)
NETLNS	?	−0.0038	(−1.06)	0.0079	(0.49)
PROFIT	?	−0.0385	(−0.65)	−0.2866	(−0.77)
MVLEV	?	0.0000	(−0.19)	0.0020	(2.42)**
SIZE	–	−0.0013	(−2.83)***	−0.0006	(−0.35)
STDRET	+	0.1736	(2.24)**	0.3363	(0.87)
PRICE	–	−0.0001	(−1.35)*	NA	NA
NASDAQ	+	0.0013	(1.25)	NA	NA
TURNOVER	–	−0.0131	(−2.47)**	NA	NA
FOLLOW	–	−0.0015	(−1.39)*	−0.0095	(−2.76)***
DERIVATIVE	+	0.0002	(2.31)**	0.0005	(1.85)**
CHGOFF_ONBS	+	−0.1370	(−1.41)	−0.1002	(−0.16)
NPL_ONBS	+	0.0257	(0.47)	0.2070	(0.97)
GAP	+	0.0018	(0.80)	0.0043	(0.33)
Adj-R^2		59.29%		27.10%	
n		871		687	

Panel C: Using Size (*SIZE*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	t-statistic	Coefficient	t-statistic
Intercept	?	0.0263	(5.03)***	0.0159	(0.75)
RECOURSE	+	0.0055	(1.81)**	0.0450	(2.19)**
*RECOURSE * SIZE*	–	−0.0004	(−1.42)*	−0.0038	(−1.96)**
*RECOURSE * STDRET*	?	−0.0142	(−0.48)	0.0099	(0.21)
NETLNS	?	−0.0045	(−1.30)	0.0024	(0.18)
PROFIT	?	−0.0265	(−0.37)	−0.2631	(−0.74)

(continued on next page)

Panel C: Using Size (*SIZE*) as a Measure of the Quality of the Information Environment

	Predicted Sign	Bid-Ask Spread		Forecast Dispersion	
		Coefficient	**t-statistic**	**Coefficient**	**t-statistic**
MVLEV	?	0.0001	(0.77)	0.0018	(2.82)***
SIZE	–	−0.0017	(−3.21)***	−0.0010	(−0.57)
STDRET	+	0.1536	(1.75)**	0.3350	(0.85)
PRICE	–	−0.0001	(−1.33)*	NA	NA
NASDAQ	+	0.0018	(1.55)*	NA	NA
TURNOVER	–	−0.0148	(−2.92)***	NA	NA
FOLLOW	–	−0.0019	(−2.05)**	−0.0041	(−1.38)*
DERIVATIVE	+	0.0003	(2.62)***	0.0006	(2.63)***
CHGOFF_ONBS	+	−0.1251	(−0.94)	−0.2050	(−0.30)
NPL_ONBS	+	0.0933	(1.18)	0.0291	(0.14)
GAP	+	0.0043	(1.77)**	0.0006	(0.04)
Adj-R^2		55.84%		24.45%	
n		871		687	

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*, **, *** Denote statistical significance at 10 percent, 5 percent, and 1 percent, respectively, based on one-tailed tests if the coefficient has a predicted sign, and two-tailed tests otherwise.

Table 5 presents results from regressions of banks' bid-ask spreads and analyst forecast dispersion on the recourse uncertainty factor, the recourse uncertainty factor interacted with information environment quality proxies and with bank intrinsic risk, as well as control variables. The analyses in Panels A, B, and C use the number of analysts following, the percentage of institutional holdings, and the firm size, respectively, to measure the quality of the information environment. Standard errors are cluster-adjusted by firm and quarter following Petersen (2009) and Gow et al. (2010).

See the Appendix for variable definitions.

information users protect themselves against high uncertainty by assuming minimal risk transfer for all securitized assets (i.e., assuming that all securitizations are, in fact, secured borrowings).[20] In this case, we would expect no difference in information uncertainty between securitizing and non-securitizing banks. Given these opposite expectations, it is unclear *ex ante* whether the net effect of securitization would be an increase, a decrease, or no change in information uncertainty relative to non-securitizing banks.

To assess the effect of securitization on information uncertainty, we match our sample of securitizing banks by size, risk level (i.e., standard deviation of returns), and quarter with a sample of non-securitizing banks. This matching allows us to investigate whether, holding constant important bank characteristics such as size and risk, the propensity to undertake securitization transactions affects bank information uncertainty. We then regress each of the two information uncertainty proxies (the bid-ask spread and the analyst forecast dispersion) on an indicator variable (*ABSD*) that takes a value of 1 (0) for securitizing (non-securitizing) banks and the control variables used in Equation (1). The results from this analysis (untabulated) indicate that the coefficients for *ABSD* in both the bid-ask spread and the forecast dispersion models are positive and statistically significant. These findings suggest that securitizing banks have higher levels of information uncertainty as compared to banks that do not use this financing technique.

It is possible that a bank's level of information uncertainty and its securitization activities are endogenously determined. The information uncertainty faced by banks could also influence their decision of whether to securitize. The literature cites immediate access to capital and the diversification of funding sources as some of the main benefits of securitization. To the extent that banks with high levels of information uncertainty find it more difficult and/or costly to access other sources of capital, they may be more likely to undertake asset securitization transactions to finance their operations.

To control for this possible endogeneity problem, we estimate a two-equation system, wherein we model the decision to securitize assets and the bank information uncertainty simultaneously.[21] Hausman tests reject the null hypothesis of no endogeneity for both our measures of information uncertainty. Our (untabulated) findings based on this system of equations provide support for the hypothesis that securitizing banks have higher information uncertainty (specifically, higher bid-ask spreads and analyst forecast dispersion) compared to non-securitizing banks.[22]

VI. SUMMARY AND CONCLUSION

In this study, we investigate the effect of bank asset securitization on information uncertainty. We first hypothesize that, for securitizing banks, difficulty in estimating securitization recourse can lead to higher information uncertainty among investors with different access to information and different abilities to process information. We then investigate whether this effect can be mitigated in settings where the quality of the information environment is high. Finally, we compare the information uncertainty levels between securitizing and non-securitizing banks, after controlling for other determinants of information uncertainty and for endogeneity.

We find that our empirical proxies for the difficulty in assessing recourse are positively associated with bank information uncertainty. However, this effect is mitigated for securitizing banks that operate in a higher quality information environment (as measured by larger analyst following, larger institutional ownership, and greater bank size). Our findings also indicate that banks that engage in securitization transactions have higher bid-ask spreads and analyst forecast dispersion compared with banks that do not engage in this type of financing transactions.

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[20] Prior theoretical studies (e.g., Cao et al. 2005) show that investors will adopt a worst-case scenario view when faced with large levels of uncertainty.

[21] While in our empirical tests of comparing securitizing and non-securitizing banks we attempt to control for bank-relevant characteristics, we cannot completely rule out the possibility that unobserved (and uncontrolled for) differences between securitizing and non-securitizing banks are driving our findings.

[22] The results for both the matched sample analysis and the 2SLS analysis are available from the authors on request.

This study contributes to the literature by investigating the implications of securitization transactions and securitization recourse on information uncertainty. Our findings contribute to a better understanding of how market participants differ in their assessment of complex transactions such as securitizations. Specifically, our findings imply that inadequate disclosures can lead to higher information uncertainty for securitizing banks, and that higher information quality can help mitigate this uncertainty. These findings provide support for FASB's recent efforts to improve securitization disclosures (FASB 2008; ASC 860-10, formerly SFAS No. 166, FASB 2009a, 2009b).

APPENDIX
VARIABLE DEFINITIONS

SPREAD = average of daily effective bid-ask spreads during quarter t, where the effective bid-ask spread is measured as two times the absolute value of the difference between the daily ending trading price and the average of the ask and bid scaled by the average of bid and ask (from TAQ consolidated trades and consolidated quotes files);

DISPERSION = one-year-ahead analyst forecast dispersion calculated in the middle of the second month during quarter t, where dispersion is measured as the standard deviation of forecasts scaled by mean of forecasts (from I/B/E/S summary file);

ABSD = an indicator variable that takes the value of 1 if a firm has any securitized assets at the end of quarter t, and 0 otherwise;

NETLNS = total loans, net of the allowance for loan and lease losses plus customers' liabilities on outstanding acceptances scaled by total assets at end of quarter t;

PROFIT = ratio of net income to total assets at the end of the quarter t;

MVLEV = ratio of total liability to book value of equity at the end of quarter t;

SIZE = log of total assets at the end of quarter t;

STDRET = return volatility measured as the standard deviation of daily returns during quarter t;

PRICE = price per share at the end of quarter t;

NASDAQ = dummy variable that takes the value of 1 if a firm is listed on the NASDAQ, and 0 if the firm is listed on the NYSE or the AMEX;

TURNOVER = average daily turnover during quarter t, where turnover is measured as trading volume scaled by market capitalization at the end of each day (from the CRSP daily stock file);

FOLLOW = log of 1 plus the number of analysts following measured in the middle of the last month during quarter t (from the I/B/E/S summary file); if a firm is not covered by an analyst, *FOLLOW* = 0;

DERIVATIVES = total notional amount of derivative contracts scaled by total assets at the end of quarter t;

CHGOFF_ONBS = charge-offs on balance sheet loans scaled by total assets at the end of quarter t;

NPL_ONBS = past due on balance sheet loans scaled by total assets at the end of quarter t (where the past due loans equal loans past due 30 through 89 days plus loans past due 90 days or more);

(continued on next page)

GAP = absolute value of the difference between assets and liabilities expected to reprice within the next 12 months scaled by total assets at the end of quarter *t*;
ABS = total securitized assets scaled by total assets at the end of quarter *t*;
SECINC = securitization income and servicing fees scaled by total net income for quarter *t*;
NPL_SEC = total nonperforming securitized loans scaled by total assets at the end of quarter *t*;
CHOFF_SEC = total charge-offs for securitized loans scaled by total assets at the end of quarter *t*;
RET_INT = total retained interest from all asset securitizations scaled by total assets at the end of quarter *t*; and
RECOURSE = first factor from a principal component analysis using *ABS, SECINC, NPL_SEC, CHOFF_SEC*, and *RET_INT*.

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Warranty Reserve: Contingent Liability, Information Signal, or Earnings Management Tool?

Daniel Cohen
The University of Texas at Dallas

Masako N. Darrough
Rong Huang
Baruch College–CUNY

Tzachi Zach
The Ohio State University

ABSTRACT: We examine the information role of accounting disclosures on warranties, utilizing a database that became available due to the requirements of FIN 45. First, because firms use warranty policies as a business strategy to promote their products, a warranty reserve can serve two roles: an information signal regarding product quality, as well as a contingent liability. Consistent with this view, we find that the stock market recognizes that: (1) the warranty reserve contains information about firms' future performance, and (2) the reserve is a liability. Second, because warranty accruals require estimation of future claims, they can be used as a tool of earnings management. Our evidence indicates that managers use warranty accruals to manage earnings opportunistically to meet earnings targets. Finally, we find that the stock market recognizes the understatement of warranty liabilities of firms that managed earnings.

Keywords: *warranties; contingent liability; product quality; signaling; earnings management.*

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I. INTRODUCTION

Most durable products are sold with warranties. A product warranty is "an obligation incurred in connection with the sale of goods or services that may require further performance by the seller after the sale has taken place" (SFAS No. 5, *Accounting for Contingencies*).[1] A warranty guarantees customers that a product will provide expected service. In the event of failure, the warranty provider would rectify the product according to the terms of the warranty policy, which can vary in duration and scope (full or limited, labor and/or parts, repair versus refund, etc.). Thus, a warranty can reduce uncertainty about the product's future performance, providing insurance against failure (Heal 1977). More importantly, warranties can be a means of signaling product quality by producers when information asymmetry exists between producers and consumers (e.g., Spence 1977; Grossman 1981; Lutz 1989).

The accounting aspects of product warranties have yet to be studied. Our study helps to fill this void by investigating the role of accounting information on warranties. We use a comprehensive database of warranty disclosures that has not been available to researchers until recently. Although firms were at liberty to disclose warranty information voluntarily, Financial Interpretation No. (FIN) 45, which took effect in 2003, mandated the disclosure of such information.

We address two research questions. First, how does the capital market interpret accounting information on warranties? Specifically, we ask whether the market interprets warranty reserves as an information signal or a contingent liability.[2] Second, we ask whether firms use warranty accounting as an earnings management tool. To address these questions, we employ a sample of 806 firms that disclosed quarterly warranty information from 2003 to 2006. We supplement the data with hand-collected information on warranty durations from firms' annual reports for a subsample of 145 firms.

Our first research question examines the market valuation of warranty reserves. If firms provide warranties as insurance, then warranty liabilities are simply contingent liabilities: future obligations to perform services if a product fails. Accordingly, one dollar of warranty liabilities would be expected to reduce firm value by one dollar. However, if firms offer warranties to signal product quality, then warranty liabilities can have an additional role in providing information on firm value and future firm performance. Due to this dual nature of warranty liabilities, we expect them to differ from other liabilities such as bank loans.

Our results demonstrate that the stock market values warranty liabilities differently from other liabilities by placing a smaller negative valuation coefficient on them. However, after controlling for analysts' earnings growth expectations and the duration of warranties, the valuation coefficients on both warranty liabilities and other liabilities approach -1. We also show that the valuation coefficient increases as firms provide better warranty coverage. However, within a group of firms with the same coverage, the valuation coefficient on warranty liabilities is negative. In addition, we document a significant positive relation between abnormal warranty expense and future firm performance across firms with different warranty terms, and a negative relation within groups of firms that provide the same warranty terms. Collectively, these findings suggest that warranty liabilities are not merely a tool of insurance; rather, they reflect a signal about firm types and future performance. Moreover, the market appears to understand this information role of warranty liabilities.

Our second research question examines whether managers use warranty accounting as a means of opportunistic earnings management to meet short-term financial reporting objectives.

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[1] Warranties can be either "express" or "implied." An express warranty typically contains a written warranty policy detailing the terms of warranty. An implied warranty is an implicit understanding that the product meets the warranty of merchantability, i.e., fit for sale and consumption as represented at the time of sale.

[2] Throughout the study we use the terms "warranty reserve/s" and "warranty liability/ies" interchangeably.

These opportunistic accounting decisions can be achieved through changes in the assumptions and estimates underlying warranty accruals. Achieving earnings targets, such as avoiding losses, avoiding earnings decreases, and meeting or beating analysts' forecasts, has been extensively studied in the accounting literature (e.g., Burgstahler and Dichev 1997; Degeorge et al. 1999). In general, the consensus from prior research is that managers care greatly about these benchmarks and are willing to engage in costly earnings management strategies to achieve them (e.g., Brown and Caylor 2005; Graham et al. 2005). We show that firms that achieve earnings targets report significantly lower warranty expenses than their counterparts. This evidence is consistent with managers using the flexibility in the assumptions underlying the calculation of warranty expenses to achieve specific financial reporting objectives.

Our final analysis combines the valuation and earnings management aspects. We show that, after controlling for both the information role of warranty reserves and earnings management incentives, the market views warranty liabilities similarly to other liabilities. Also, firms that use warranty accruals to achieve earnings targets have a stronger negative valuation coefficient on their warranty liabilities. This suggests that investors recognize that the warranty liabilities of these firms are understated.

We contribute to the existing accounting literature in several ways. First, we extend prior research on the role of accounting information by examining how the capital market evaluates warranty information. We take advantage of the unique economic nature of warranty information, and document that warranty liabilities play a dual role: as contingent liabilities and as signals of product quality and future earnings growth. Second, in the context of earnings management, we delve deeper into warranty accruals. Thus, we are able to: (1) increase the power of the analysis, and (2) move away from models that evaluate aggregate discretionary accruals to a more specific model that allows us to capture the richness of the warranty setting. In that, we specifically answer the calls made by accounting researchers (for example, McNichols 2003) for disaggregating empirical measures of accounting choices. In so doing, we advance the literature on earnings management and shed light on the specific methods that managers use to achieve earnings targets. Third, our results imply that the mandated disclosures of FIN 45 regarding warranties are useful and relevant to capital market participants.

In Section II we provide a brief background on the economic role and accounting treatment of warranties. We also outline the theory that underlies our hypotheses, which we develop in Section III. In Section IV we describe our sample and empirical methodologies. We report results in Section V and conclude in Section VI.

II. BACKGROUND

The Economic Role of Warranties

In the U.S., issuing warranties for consumer products has its roots in the automobile industry (see Burton 1996). As the number of automobile companies grew, they started to use warranties as a means of competition. Other manufacturers then began offering warranties as a standard practice. Ambiguities in these contracts, however, presented enforcement problems. In order to achieve a uniform standard in warranty contracts, Congress passed the Magnuson-Moss Act in 1975. The Act required that warranty plans (when offered) explicitly describe the scope and duration of coverage, the means to obtain warranty services, and how various state laws on warranties are affected. In addition, warranties are to be offered before consumers buy a product. The Act states: "The disclosure requirement's purpose was to insure that a warranty would be a useful gauge—or signal—of a product's reliability."[3]

With the advent of disclosure requirements, warranties became an increasingly important strategic mechanism for manufacturers. The economics literature argues that warranties are a means to overcome information asymmetries regarding product quality between an informed manufacturer and uninformed customers. By issuing a warranty plan that depends on an *ex post* verifiable outcome that is correlated with product quality, the manufacturer bonds herself (and the buyer protects himself) to its product quality (Grossman 1981). Spence (1977) posits that manufacturers provide warranties with better terms to signal their product quality.[4] In the signaling context, product quality refers to products' attributes that are not easily visible to buyers, rather than to observable characteristics. Sellers turn to costly warranty plans to convince buyers that their products are of better quality and their firms are of better "type."[5] Consistent with this notion, researchers in marketing found that product reliability and warranties' duration are positively related for a wide range of consumer durable products (Gerner and Bryant 1978; Wiener 1985; Kelley 1988). In an experimental setting, Boulding and Kirmani (1993) found that consumers perceive that a better warranty implies a higher quality product.[6]

Accounting for Warranties

Manufacturers who provide product warranties are required to accrue a warranty expense at the time of sale.[7] Like many other accruals, warranty expenses are estimated based on company's projections of future claims. Warranty expenses are an important selling expense and can be substantial. In our sample, the average warranty expense constitutes about 1.4 percent of sales and 11 percent of operating income.

The disclosures of warranty expenses and liabilities were voluntary until the issuance of FIN 45, *Guarantor's Accounting and Disclosure Requirement for Guarantees, Including Indirect Guar-*

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[3] U.S. House Committee on Interstate and Foreign Commerce (1970), *Hearings on Warranties and Guarantees*, 91st Cong., 2d sess.

[4] Another view on warranties in the economics literature is that warranties facilitate risk-sharing between sellers and buyers (Heal 1977). Sellers and buyers might be aware of the failure rate (i.e., no information asymmetry about product quality), but it may be impossible to determine if a specific item is a lemon. If warranties are provided as insurance, then differences in warranty plans mainly reflect different consumers' attitudes toward risk. In addition, the terms of warranty plans might specify the conditions under which the plan is honored, thereby promoting proper use of the product. Consumers would value products with warranties more and would be willing to pay higher prices for them. Costs of servicing warranties are additional product costs, while warranty liabilities represent contingent liabilities.

[5] Although the positive relation between product quality and warranty quality is intuitively appealing, it is by no means the only theoretical prediction in signaling games. For example, Lutz (1989) derives a separating equilibrium in which high product quality is signaled with a low warranty plan and a low product price when consumers are subject to moral hazard. Under double moral hazard (both consumers and producers), the relation between warranty policy and firm type can be either positive or negative, depending on the parameter values (Cooper and Ross 1985). Gal-Or (1989) analyzes the role of a warranty in an oligopolistic market and shows that multiple equilibria can result; warranty quality/firm-type relation is positive in one equilibrium, but negative in another. Thus, in these equilibria the information content of a warranty plan regarding firm type is extremely limited.

[6] Warranties can also be used as a marketing tool. As such, warranties reflect a firm's strategy to establish or improve its reputation among its customers. If customers infer that a company providing products with better warranty coverage is more reliable than a company providing less warranty coverage, then offering a better warranty could be an effective strategy (Murthy and Djamaludin 2002). In addition, firms may use warranties to promote future sales and growth even though it is costly to do so (Menezes and Quelch 1990).

[7] Sellers sometimes offer *extended* warranties for additional fees. Under the current accounting regulation (Technical Bulletin 90-1), revenues from extended warranties are deferred and service costs are expensed as incurred. Thus, accounting information on warranties does not include information on extended warranties. While GAAP requires warranty expense to be matched against the revenues of the products to which the warranty applies, for tax purposes the deduction is allowed only when the costs of providing the warranty services are actually incurred. Accordingly, this treatment may give rise to a deferred tax asset.

antees of Indebtedness of Others, in 2002 (FASB 2002).[8] By mandating disclosures, FIN 45 expands the information made available to investors about firms' warranty accruals, claims, and liabilities. Beginning in 2003, firms provide: (1) the estimated amount of future payments under the warranty plan (warranty liabilities), (2) the accounting policy and methodology used in determining warranty liabilities, and (3) a tabular reconciliation of the changes in the warranty liability for the reporting period. This reconciliation presents the beginning balance of the warranty liability, the reductions in that liability for payments made under the warranty plan (i.e., claims), the changes in the liability for accruals (i.e., warranty expenses) related to product warranties issued during the reporting period, the changes in the liability for accruals related to preexisting warranties (including adjustments related to changes in estimates), and the ending balance of the warranty liability. Appendix A provides two examples of warranty disclosures from the financial statements of Dell and Western Digital.

Interpretation of Warranty Data: A Signaling Perspective

Possible Equilibria

We now discuss how one could interpret the accounting information on warranties (expenses, claims, and liabilities) from a signaling perspective in which a firm designs its warranty policy to signal its firm "type" (product quality). Although the direct signaling mechanism is the warranty policy itself, accounting information on the warranty plan could also reflect firm type and provide supplementary information.

Consider the three possible equilibria in a signaling game: pooling, fully separating, and partially pooling equilibria. Assume first that warranty policies are observable without noise. If a pooling equilibrium prevails, then one cannot differentiate firm type from observing identical warranty policies. But accounting information on warranties can reveal firm type; inferior quality will result in higher claims and higher warranty expenses. In this case, quality and warranty costs[9] are negatively related.[10]

Next, consider a fully separating equilibrium in which higher-quality types provide warranty policies with better coverage.[11] In this case, warranty policies signal product quality and reveal firm type. Although accounting information reflects the cost of providing the signal, it does not provide any incremental information about firm type. Finally, in the case of a partially pooling equilibrium, warranty cost information is informative about firms within each pool, but does not provide any incremental information across pools. Again, warranty policies themselves fully reveal firm type across policy pools.

In sum, if warranty policies are observed *perfectly*, then accounting information provides incremental information about firm type only within each pool, but not across pools. However, because warranty plans are likely to be observed with noise, even for a separating equilibrium, accounting information is likely to provide incremental information.[12] Therefore, we expect accounting information on warranties to be value-relevant regardless of the prevailing equilibrium.

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[8] FIN 45 applies to financial reports ending after December 15, 2002. Prior to FIN 45, the disclosure on warranty obligations were voluntary unless the warranty liabilities exceeded 5 percent of total liabilities. Gu (1998) documents that in that era, firms differed in their voluntary disclosure behavior with respect to warranty information.

[9] By warranty costs, we mean costs of providing warranty services. Warranty expenses are estimates of warranty costs accrued at the time of sale.

[10] One should note, however, that while firms could offer identical warranty plans, a pooling equilibrium is sustainable only if firms in the pool are similar. Since buyers will be able to infer the quality of the products by examining warranty expenses (i.e., the higher warranty expenses, the lower the product quality), a lower quality firm is likely to reduce the level of warranty plans. Thus, in the long run, better firms are more likely to offer better plans.

[11] For simplicity, we assume that warranty coverage can be characterized by its duration and scope, and that buyers are able to assign a strict preference ordering over various plan features.

[12] There are two sources of noise. First, firms typically provide only coarse information on warranty policies, such as the range of warranty duration for their products. Even though we develop a method to evaluate various features of warranty

How are warranty costs and firm type related? As discussed earlier, in a pooling equilibrium in which several types send the same signal, warranty costs and type are negatively related. The relation in a separating equilibrium, however, requires further discussion.

Relation between Warranty Costs and Firm Type in a Separating Equilibrium

We first illustrate how a separating equilibrium emerges. Consider a high-quality car manufacturer. While potential buyers can observe some characteristics of the car (e.g., design, paint, engine size, performance specifications, sunroof, leather seats), they cannot observe the car's reliability. Knowing that the car is reliable, the car manufacturer offers an extensive warranty to signal its quality type. Although the car manufacturer will have higher warranty costs, the superior warranty plan will increase the demand for the car, resulting in a higher selling price and/or sales volume, which will generate higher profits. Less reliable car makers, however, find it too costly to mimic the high-reliability car maker.

For such a scenario to be an equilibrium, there must be a cost structure in which the marginal cost of providing better coverage is lower for a higher type than for a lower type (referred to as the "single crossing property"). Thus, a lower type cannot mimic a higher type (self-selection). That firm type and warranty plan are positively related in equilibrium, however, does not imply that firm type and warranty *costs* are positively related. In Appendix B, we formally demonstrate that if the cost advantage of a higher type over a lower type (two adjacent types) in providing warranties is not too large, then warranty costs and type are positively related in a separating equilibrium.

In selecting a warranty plan, a higher type trades off a higher revenue generated from a better warranty plan against its higher cost. Even though the higher type has a cost advantage over the lower type in providing the same warranty plan, the higher type's cost will be higher for a better plan than for an inferior plan. If the cost advantage is not too large, however, then the warranty cost of a better plan by a higher type is greater than the warranty cost of an inferior plan by a lower type. Thus, unless the higher types have a significant technological advantage in providing warranty plans over lower types, it is more likely that warranty costs and type are positively related in equilibrium. In most industries with many producers, it is unlikely that any firm commands such a technological advantage in providing warranty services.

The relation between warranty liabilities and product quality is determined by claim rates, timing of claims (how soon products fail and how fast consumers submit claims), and the duration of warranty coverage as well as accrued warranty expenses. *Ceteris paribus*, warranty liabilities will expire more quickly as the duration decreases and more slowly as the duration increases. Thus, for firms with similar claim patterns, we expect warranty expenses and warranty liabilities to be positively correlated.

To summarize, in a separating equilibrium, we expect a positive relation between warranty expenses and firm type as well as between warranty liabilities and firm type, if the differences in the costs of providing warranties between high-type and low-type manufacturers are not too large. Further, in a partially pooling equilibrium, we expect a positive relation between costs and firm type across different pools of firms, but a negative relation within each pool.

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plans for a specific product and to assign a score for the warranty plans, most of these features may not be easily observable. Second, since most firms sell many products, one needs information on the warranty policies and the sales levels of all products sold by the firm in order to compute an aggregate measure for each firm. However, such disaggregated data are not available.

III. HYPOTHESES DEVELOPMENT

We now develop specific hypotheses for our empirical analyses. The first two hypotheses focus on how the capital market evaluates warranty liabilities (H1), and on the relation between warranty expenses and future firm performance (H2). In these hypotheses, we view warranty policies as part of an overall business strategy and assume that firm type (product quality) is positively related to both firm value and future performance.[13] The third hypothesis (H3) relates to the accounting choices regarding warranties and whether managers use them to manage earnings to achieve targets.

Signaling Role of Warranty Information

Because of the uncertainty associated with future claims, a product warranty falls under the definition of a contingent liability. Valuation of contingent liabilities involves assumptions and estimates that are unobservable by outsiders. Studies that investigate contingent liabilities' valuations generally find that these liabilities are negatively associated with firm value.[14]

With respect to warranty liabilities, we expect that if they do not contain any information regarding product quality and firm performance, then they will be negatively related to firm value, like other liabilities. In this case, the valuation coefficient placed on warranty liabilities is expected to equal the coefficient on other liabilities. Therefore, our first hypothesis is as follows:

H1a: The valuation coefficient placed on warranty liabilities is equal to the valuation coefficient placed on other recognized liabilities.

If, however, warranty liabilities capture the warranty policies' information on product quality, and investors recognize the dual nature of warranty liabilities, then the coefficient on warranty liabilities is likely to be less negative than the coefficient on other liabilities. We predict that the coefficient on warranty liabilities will approach that of other liabilities only when we control for the signaling role of warranty liabilities.

Perusal of warranty policies of our sample firms shows that warranty coverage varies within an industry, especially in terms of duration. However, there are clusters of firms with the same warranty duration, defined as warranty pools.[15] That is, many industries in our sample exhibit partially pooling equilibria. As discussed earlier, the relation between warranty liabilities and product quality in a partially pooling equilibrium depends on whether we are concerned with firms within a pool or across pools. Formally:

H1b: Warranty liabilities increase with firm type across pools, while they decrease with firm type within a pool.

Because firms in better pools are likely to perform better in the future, we expect the market to value the signaling component of their liabilities positively. More specifically, we expect the coefficients in our valuation models to increase across pools. Within each pool, however, we expect higher warranty liabilities to reduce valuation.

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[13] Our assumption finds support in the literature. Ittner and Larcker (1998) show that product quality increases customer satisfaction, which in turn increases firm's profitability. Nagar and Rajan (2001) document a negative relation between product defects and future sales. The literature on the Balanced Scorecard also discusses the relation between future performance and product quality as one form of nonfinancial performance measures (Kaplan and Norton 1992, 1996).

[14] These include the studies of pensions (e.g., Barth 1991; Espahbodi et al. 1991; Landsman 1986), retirees' health benefits (Mittelstaedt and Warshawsky 1993), bank loan loss provisions (Petroni 1992; Wahlen 1994; Liu et al. 1997), and environmental liabilities (Barth and McNichols 1994).

[15] In this study, we use the information on duration of warranty coverage as the proxy for the quality of warranty plans. While there are other attributes to characterize warranty coverage, duration is the most common way to summarize warranty policies. For example, the Magnuson-Moss Act states: "Only when the rules of the warranty game are clarified... so that the duration of the warranty is a useful comparative gauge of product reliability ... [will it be] economically rewarding for producers of consumer products to build reliability into their products." In addition, all warranty policies must specify warranty duration. Thus, most studies on warranties use duration as a summary measure for the quality of policies.

Similar to the valuation context, we expect the relation between warranty expenses and future performance to vary according to the set of firms we examine. Thus:

> **H2:** Warranty expenses increase with firm type across pools, while they decrease with firm type within a pool.

Our empirical tests for examining H2 focus on the impact of unexpected increases in warranty expenses on future firm profitability.

Benchmark Beating and Warranty Accruals

Our third hypothesis concerns the relation between warranty accounting choices and managerial incentives to meet or beat earnings benchmarks. The means by which managers achieve earnings targets are numerous and broadly include accrual management and real earnings management.[16] Several studies examine specific accrual choices and find some evidence of earnings management. By limiting attention to a specific accounting choice, these studies are able to potentially increase the power of the tests.[17] McNichols (2003) emphasizes the importance of disaggregating empirical measures of accounting choices to generate a more powerful setting. The warranty context enables us to overcome some of the difficulties posed by aggregate, accrual-based measures and to directly address the call by McNichols (2003).

If firms use warranty expenses to achieve financial reporting objectives, then there should be an association between abnormal warranty expenses and variables proxying for reporting incentives. We focus on three extensively studied earnings benchmarks: (1) avoiding reporting a loss, (2) avoiding reporting an earnings decrease, and (3) meeting analysts' forecasts.

For each of the three benchmarks, we define "suspect" firms as those firms that are more likely to have used warranty expenses to meet one of the three benchmarks. Specifically, we identify firms whose pre-managed earnings numbers fall short of the target benchmark, but whose post-managed numbers exceed the targets. Abnormal warranty expenses are used to compute pre-managed earnings. Thus, we compare abnormal warranty expenses of these firms to those of a set of non-suspect firms. Our third hypothesis, stated in alternative form, is:

> **H3:** Firms that were just able to exceed an earnings benchmark will report lower abnormal warranty expenses for that quarter compared to other firms.

IV. SAMPLE AND EMPIRICAL METHODOLOGY

Data

FIN 45 introduced new disclosures about warranty accruals, claims, and liabilities. We obtain these data for the years 2003–2006 from *Warranty Week* (http://www.warrantyweek.com). The sample firms are drawn from the set of manufacturing firms that are expected to have significant warranty expenses. We also hand-collect information about the duration of warranties from the 10-K filings of a subset of our sample firms that belong to industries with more than ten firms in our sample.

We describe our sample construction in Table 1. The original file contains 14,224 firm-quarter observations covering 889 unique firms. Of these, we eliminate 832 observations belonging to 52

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[16] Another way to achieve the important benchmark of beating analysts' forecasts is by managing analysts' expectations (Matsumoto 2002).

[17] For example, Beaver et al. (2003) study the loan loss reserves in property-casualty insurance companies. Beatty et al. (2002) provide evidence that public banks reduce loan loss reserves to avoid reporting earnings declines. See also Moehrle (2002) and Dhaliwal et al. (2004).

TABLE 1
Sample Composition

	Firm-Quarters	Firms	Firm-Quarters	Firms
Full Sample				
Original file	14,224	889		
Observations without valid Compustat GVKEY information	(832)	(52)		
Observations of firms without sales information in some quarters during the sample period	(1,469)			
Observations of firms without direct information on warranty expenses and claims in some quarters during the sample period	(1,998)			
No warranty data for any quarter	(404)	(31)		
	9,521	806	9,521	806
Valuation Analysis				
Missing information on warranty liabilities, total assets, and analysts' growth forecasts	(3,653)	(125)		
	5,868	681		
Missing abnormal warranty expenses (industry model)	(903)	(50)		
	4,965	631		
Earnings Management Analysis				
Missing abnormal warranty expenses (industry model)			(3,991)	(142)
			5,530	664
Missing ΔROA_{t+1}, ΔROA_t and sales growth			(1,036)	(78)
			4,494	586
Subsample with *TERM* Information				
Full Sample			5,868	681
Observations belong to four-digit SIC code industries with less than ten firms			(2,092)	(290)
Observations without disclosure of warranty term information in 2006 10-K filings or the disclosure is ambiguous			(2,082)	(220)
Missing *BM*, *LIFE*, and *AGE*			(200)	(26)
			1,494	145

firms for which we could not obtain valid Compustat identification information. Following several additional data requirements, the number of effective firm-quarter observations with warranty data in our analyses is 9,521.[18] However, the number of observations in our tests may be smaller depending on model-specific variable requirements, some of which are listed in Table 1. We also conduct additional analyses on a subset of firms for which we obtain information about the duration of warranties (*TERM*). We require that these firms belong to industries with at least ten firms to ensure that we obtain a reliable benchmark against which to evaluate each firm's warranty term. This requirement as well as the existence of information about warranty duration, reduces the sample in these analyses to 1,494 observations spanning 145 firms.

The sample firms originate from several industries, but as manufacturing firms, they are concentrated in a number of groups. As reported in Table 2, about 70 percent of the sample firms belong to three industry groups: industrial machinery and equipment (196 firms; 24.3 percent), electronic and other electric equipment (198 firms; 24.6 percent), and instruments (165 firms; 20.5 percent). Warranty expenses in these industries range between 1.45 percent and 1.82 percent of sales. The last column in Table 2 reports the median warranty duration (in years).

Variable Definitions

In our analyses we use three proxies for quarterly abnormal warranty expenses and abnormal claims. Our first proxy is based on the seasonal change in warranty expenses or claims, adjusted for the seasonal change in sales. In calculating this proxy, we assume that the level of warranty expenses (or claims) for firm i is proportional to its sales in quarter t, i.e., $WEXP_{i,t} = \alpha_{i,t} SALES_{i,t}$, where $\alpha_{i,t} = WEXP_{i,t-4} / SALES_{i,t-4}$. Thus, abnormal warranty expenses in our time-series seasonal model (*ABWEXP*) are:

$$ABWEXP\_TIME_{i,t} = \frac{WEXP_{i,t} - WEXP_{i,t-4} * \dfrac{Sales_{i,t}}{Sales_{i,t-4}}}{TA_{i,t-4}}. \qquad \text{(Time-series model)}$$

We obtain observations of each variable in quarter t and use the same variable in the same quarter in the previous year (t–4) as a benchmark to account for the seasonal change in product mix. We control for growth in a firm's operations, which is one of the important determinants of warranty accruals. Marquardt and Weidman (2004) utilize a similar model in a different context. We compute the abnormal claims (*ABCLAIM*) made during a particular quarter in the same way by replacing *WEXP* with *CLAIMS*. This is a more direct measure of changes in product quality.

Our second proxy is an industry-adjusted measure based on the membership in a common two-digit SIC code group. For each quarter, we compute the mean level of the ratio of expenses (or claims) to sales, excluding the firm for which we calculate the measure. We require at least ten firms in the industry group. We consider the deviation from the industry mean as our proxy for the industry-adjusted abnormal warranty expenses. Thus, abnormal warranty expense in our industry model is:

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[18] We delete 1,469 observations with missing sales information in some quarters and an additional 2,402 observations that do not have direct information on warranty expenses. Warranty expenses are missing due to one of several reasons, including: firms that adopted FIN 45 after 2003, firms that only provide annual disclosures, and firms that have warranty policies but do not comply with the FIN 45 requirement. Based on these categories, we conclude that most, if not all, of our sample firms do provide warranties to their products. Therefore, filling out the missing quarters with zeros would be inappropriate.

TABLE 2
Sample Composition by Industry

SIC Code (two digits)	Industry	n	n (%)	*WEXP/SALES* (%)	*CLAIM/SALES* (%)	Duration in Years (Median)
14	Mining and Quarrying Non-Metallic Minerals	1	0.12	0.020	0.133	—
15	General Building Contractors	24	2.98	0.750	0.617	—
16	Heavy Construction, Except Building	1	0.12	1.205	0.714	—
17	Construction—Special Contractors	3	0.37	0.968	0.900	—
22	Textile Mill Products	3	0.37	1.090	1.149	—
24	Lumber and Wood Products	13	1.61	3.468	3.625	—
25	Furniture and Fixtures	20	2.48	0.612	0.597	—
26	Paper and Allied Products	1	0.12	0.065	0.053	—
28	Chemical and Allied Products	20	2.49	2.593	2.154	—
29	Petroleum and Coal Products	3	0.37	0.838	0.854	—
30	Rubber and Miscellaneous Plastics Products	12	1.49	1.079	1.109	—
32	Glass, Pottery, and Related Products	2	0.25	0.220	0.376	—
33	Primary Metal Industries	5	0.62	0.492	0.498	—
34	Fabricated Metal Products	18	2.23	0.754	0.759	—
35	Industrial Machinery and Equipment	196	24.33	1.815	2.223	1.5
36	Electronic and Other Electric Equipment	198	24.58	1.449	1.397	1.5
37	Transportation Equipment	64	7.94	1.172	1.142	2.0
38	Instruments and Related Products	165	20.48	1.550	1.426	1.0
39	Miscellaneous Manufacturing Industries	11	1.36	1.177	1.012	—
48	Communications	1	0.12	0.000	4.227	—
50	Wholesale Trade—Durable Goods	8	1.00	0.389	0.459	—
51	Wholesale Trade—Nondurable Goods	1	0.12	0.648	0.648	—
55	Automotive Dealers and Service Stations	4	0.50	0.722	0.703	—
57	Retail	1	0.12	0.000	0.057	—
63	Insurance	1	0.12	0.153	0.093	—
67	Investment Offices, Holding Offices	1	0.12	0.120	0.249	—
73	Business Services	18	2.24	0.850	0.863	—

(continued on next page)

TABLE 2 (continued)

SIC Code (two digits)	Industry	n	n (%)	*WEXP/SALES* (%)	*CLAIM/SALES* (%)	Duration in Years (Median)
75	Auto Repair, Services, and Parking	1	0.12	3.394	4.009	—
80	Services—Health	1	0.12	1.219	1.203	—
87	Engineering and Management Services	3	0.37	1.461	1.706	—
99	Non-Classifiable Establishments	6	0.74	0.705	1.714	—
		806	100.00			

$$ABWEXP\_INDUSTRY_{i,t} = \frac{WEXP_{i,t}}{SALES_{i,t}} - AVERAGE\left(\frac{WEXP_{i,t}}{SALES_{i,t}}\right)_{OTHER\_FIRMS}. \qquad \text{(Industry model)}$$

Abnormal claims are defined in the same manner.

Our third proxy considers the duration of warranties in calculating industry-adjusted abnormal warranty expenses (or claims). For each industry-quarter, we classify observations into a low-, medium-, or high-term group if the warranty duration falls below, is equal to, or exceeds the industry median, respectively. We then compute the mean level of the ratio of warranty expenses (or claims) to sales for each industry-quarter-term group, excluding the firm for which we calculate this measure. Finally, we take the deviation from the industry-quarter-term mean as our proxy for abnormal warranty expenses or claims. In addition, we use abnormal gross margin (*ABGM*) as a control variable in some of our analyses defined in an identical manner as *ABWEXP* and *ABCLAIM*.

Descriptive Statistics

Table 3, Panel A provides summary statistics that describe our sample firms. We measure all variables on a quarterly basis and report statistics over the period from the first quarter of 2003 to the fourth quarter of 2006. For some of the variables, we also provide the values for S&P 500 nonfinancial firms for comparison purposes. Our sample firms are dispersed in size, but generally smaller than the S&P 500 nonfinancial firms. The average (median) market capitalization of our sample firms is \$3.2 billion (\$678 million). The interquartile range is between \$208 million in Q1 and \$2.2 billion in Q3. The average (median) quarterly sales of our sample firms is \$639 (\$112) million. The average (median) book-to-market ratio is 0.47 (0.42), compared to 0.40 (0.35) of the S&P 500 nonfinancial firms. Our sample firms' quarterly ROA is, on average, 0.8 percent. ROA before warranty expense is on average 1.2 percent.

The average (median) warranty expense is \$8.54 (\$1.16) million, comprising about 1.4 percent of sales and 1.5 percent of total expenses. However, the average (median) ratio of warranty expenses to the absolute value of net income is 54.8 percent (13.1 percent), indicating that for many of our sample firms, the effect of managing warranty expenses could be economically significant. Finally, we find that the liability for future warranty services comprises, on average, about 4.1 percent of sample firms' total liabilities.

Table 3, Panel A further shows that abnormal warranty expenses comprise about 0.016 percent of total assets (median is 0.005 percent). The industry-adjusted warranty expense is 0.088 percent of total assets (median is 0.394 percent of total assets). The average deviation of warranty expenses from its benchmarks is small, which is not surprising because, absent product quality changes or additional factors, warranty expenses are expected to stay around the benchmark level. The average (median) quarterly warranty claims is \$7.35 million (\$1.15 million). These claims constitute about 1.3 percent of current sales. Similarly, the abnormal claims center around zero, indicating that our benchmarks are reasonable proxies of expected expenses.

Table 3, Panel B reports correlations among key variables. We focus on the warranty variables. There is a negative correlation between the fraction of warranty liabilities on firms' balance sheet and firm size, measured as either market capitalization, sales, or total assets. Further, warranty liabilities and warranty expenses are positively correlated with analysts' forecasted growth, suggesting that warranty liabilities may not only proxy for the out-of-pocket future obligations, but also contain information about future performance. We investigate this in a multivariate setting in the next section.

TABLE 3

Descriptive Statistics and Correlations

Panel A: Summary Statistics

	n	Mean	STD	Q1	Median	Q3
General Variables: S&P 500 Nonfinancial Firms (from 2003 to 2006)						
MARKET CAPITALIZATION ($ million)	6,538	20,571	38,141	4,674	9,387	18,185
QUARTERLY SALES ($ million)	6,542	3,798	7,470	744	1,729	3,623
TOTAL ASSETS ($ million)	6,534	19,469	47,272	3,550	8,582	20,181
BM	6,407	0.402	0.271	0.227	0.345	0.521
ROA	6,479	0.017	0.025	0.007	0.015	0.027
General Variables: Warranty Sample Firms (from 2003 to 2006)						
MARKET CAPITALIZATION ($ million)	4,521	3,227	9,790	208	678	2,151
QUARTERLY SALES ($ million)	4,521	639	1,807	34	112	464
TOTAL ASSETS ($ million)	4,521	2,620	8,091	137	488	1,844
BM	4,521	0.466	0.268	0.274	0.417	0.603
ROA	4,517	0.008	0.053	0.001	0.013	0.025
ROA BEFORE WEXP	4,517	0.012	0.053	0.004	0.017	0.029
ANALYST_GR (%)	4,512	16.6	8.3	12.0	15.0	19.3
Warranty-Related Variables						
WEXP ($ million)	4,521	8.541	37.927	0.252	1.155	4.770
WEXP/SALES (%)	4,521	1.377	1.336	0.479	0.962	1.863
WEXP/TOTAL ASSETS (%)	4,521	0.376	0.443	0.107	0.236	0.476
WEXP/OPINCOME (%)	4,288	10.973	152.679	1.648	5.903	14.329
WEXP/ABS(NI) (%)	4,519	54.836	306.856	5.224	13.142	32.545
WEXP/TOTAL_EXP (%)	4,247	1.478	1.438	0.494	1.024	2.048
ABWEXP$_{-time}$ (%)	4,006	−0.016	0.305	−0.092	−0.005	0.066
ABWEXP$_{-industry}$ (%)	4,521	−0.088	1.320	−0.968	−0.394	0.411
WLIAB/LIAB (%)	4,512	4.144	4.267	1.429	2.824	5.447
Claims-Related Variables						
CLAIM ($ million)	4,521	7.349	32.984	0.249	1.145	4.233

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Panel A: Summary Statistics

	n	Mean	STD	Q1	Median	Q3
CLAIM/SALES (%)	4,521	1.274	1.296	0.415	0.868	1.675
CLAIM/TOTAL ASSETS (%)	4,521	0.358	0.440	0.098	0.219	0.441
CLAIM/OPINCOME (%)	4,288	9.034	169.092	1.685	5.217	13.458
ABCLAIM$_{-time}$ (%)	4,031	−0.031	0.270	−0.094	−0.009	0.056
ABCLAIM$_{\_industry}$ (%)	4,521	−0.108	1.353	−0.975	−0.414	0.321

Panel B: Correlations

	MARKETCAP	*SALES*	*TOTAL ASSETS*	*BM*	*ROA*	*WEXP/SALES*	*ABWEXP*$_{\_time}$
MARKET CAP		**0.737**	**0.797**	**−0.160**	**0.096**	**0.002**	−0.002
SALES	**0.864**		**0.970**	**−0.049**	**0.040**	**−0.003**	0.000
TOTAL ASSETS	**0.896**	**0.958**		**−0.054**	0.021	**−0.007**	0.000
BM	**−0.311**	**−0.059**	0.015		**−0.280**	**0.001**	**0.057**
ROA	**0.297**	**0.184**	**0.077**	**−0.440**		**0.026**	0.002
WEXP/SALES	**0.701**	**0.824**	**0.784**	**−0.036**	**0.201**		0.244
ABWEXP$_{\_time}$	−0.025	−0.001	0.010	**0.082**	−0.022	**0.214**	
ABWEXP$_{\_industry}$	−0.026	0.016	−0.001	**0.064**	**0.108**	**0.901**	**0.195**
WLIAB/LIAB	**−0.225**	**−0.240**	**−0.307**	−0.012	**0.205**	**0.632**	−0.013
ANALYST_GR	**−0.275**	**−0.472**	**−0.446**	**−0.186**	0.037	**0.017**	−0.022
CLAIM/SALES	**−0.098**	**−0.089**	**−0.086**	**0.052**	**0.020**	**0.880**	0.083
ABCLAIM$_{\_time}$	−0.037	−0.022	−0.003	**0.096**	**−0.072**	**0.072**	**0.481**
ABCLAIM$_{\_industry}$	**−0.055**	−0.007	−0.012	**0.119**	0.024	**0.754**	**0.071**

	ABWEXP$_{\_industry}$	*WLIAB/LIAB*	*ANALYST_GR*	*CLAIM/SALES*	*ABCLAIM*$_{\_time}$	*ABCLAIM*$_{\_industry}$
MARKET CAP	**−0.017**	**−0.086**	**−0.112**	**−0.008**	0.006	**−0.033**
SALES	**0.008**	**−0.101**	**−0.197**	**−0.017**	0.003	**−0.009**
TOTAL ASSETS	**−0.007**	**−0.124**	**−0.188**	**−0.016**	0.009	**−0.019**
BM	**0.048**	0.008	**−0.189**	**0.046**	**0.072**	**0.090**
ROA	0.033	**0.142**	**−0.097**	**−0.064**	**−0.052**	**−0.053**

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	ABWEXP$_{\_industry}$	*WLIAB/LIAB*	*ANALYST_GR*	*CLAIM/SALES*	*ABCLAIM*$_{\_time}$	*ABCLAIM*$_{\_industry}$
WEXP/SALES	**0.940**	**0.539**	0.033	**0.861**	**0.073**	**0.793**
ABWEXP$_{\_time}$	**0.234**	0.002	−0.034	**0.083**	**0.450**	**0.074**
ABWEXP$_{\_industry}$		**0.507**	**0.028**	**0.805**	**0.076**	**0.850**
WLIAB/LIAB	**0.570**		**0.118**	**0.518**	−0.006	**0.480**
ANALYST_GR	−0.005	**0.148**		**0.012**	−0.014	0.010
CLAIM/SALES	**0.787**	**0.612**	**−0.022**		**0.206**	**0.933**
ABCLAIM$_{\_time}$	**0.074**	−0.018	−0.006	**0.173**		**0.205**
ABCLAIM$_{\_industry}$	**0.864**	**0.520**	**−0.047**	**0.859**	**0.166**	

Spearman correlations are reported on the lower left and Pearson correlations are reported on the upper right. Significance level at the 5 percent level is depicted with bold font. All variables are calculated at the end of each fiscal quarter.

Variable Definitions:
MARKET CAP = quarterly closing price multiplied by number of common shares outstanding;
SALES = quarterly sales revenue;
TOTAL ASSETS = total assets measured at the end of fiscal quarter;
BM = book value of equity divided by market value of equity;
ROA = (income before extraordinary items$_t$ + warranty expense$_t$) / *TOTAL ASSETS*$_{t-1}$;
WEXP = warranty expense;
ABWEXP = abnormal warranty expense based on either the time-series model or the industry model;
WLIAB = warranty liability;
ANALYST_GR = analyst long-term earnings growth forecasts as reported in I/B/E/S;
CLAIM = claim costs; and
ABCLAIM = abnormal claims based on either the time-series model or the industry model.

V. RESULTS

Valuation of Warranty Liabilities

We now investigate our first hypothesis regarding how warranty liabilities are related to firm's equity market prices. Our empirical specifications are derived from the Ohlson (1995) model,[19] using shares outstanding as the deflator, consistent with Barth and Clinch (2009).[20] Specifically, we estimate several variations of the following model for firm i and time t:

$$P_{i,t} = \beta_0 + \beta_1 ASSET_{i,t} + \beta_2 WLIAB_{i,t} + \beta_3 OTHER\_LIAB_{i,t} + \beta_4 ANALYST\_GR_{i,t} + \beta_5 NI_{i,t} + \beta_6 NI_{i,t}{*}Q_1 + \beta_7 NI_{i,t}{*}Q_2 + \beta_8 NI_{i,t}{*}Q_3 + Controls + \varepsilon_{i,t}, \quad (1)$$

where $P_{i,t}$ is stock price, $ASSET_{i,t}$ is total assets per share, $WLIAB_{i,t}$ is the warranty liability per share, $OTHER\_LIAB_{i,t}$ is total liabilities excluding the warranty liability per share, $ANALYST\_GR_{i,t}$ is analyst long-term earnings growth forecasts as reported in I/B/E/S, and $NI_{i,t}$ is earnings before extraordinary items per share of firm i in quarter t. To control for earnings seasonality, we include Q_1, Q_2, and Q_3 as indicators for the first three fiscal quarters. In addition, we augment the base model (1) by isolating the flow component of warranty expense (*WEXP*) and including additional control variables: *BM* is the book-to-market ratio, *AGE* is the age of the firm, computed as the difference between the current year and the year in which the business was originally formed. *LIFE* is the stage in the firm life-cycle constructed by Anthony and Ramesh (1992; see Table 4 for details). Excluding these control variables does not alter our main conclusions.

Table 4, Panel A reports the results of the market valuation of warranty liabilities.[21] The first two models serve as benchmarks to compare with subsequent regressions that incorporate warranty liabilities and growth expectations. Consistent with prior studies, the coefficient on book value per share (*BV*) in the first model is slightly greater than 1 (1.173) and the coefficient on earnings per share is positive and significant (15.228 in Q1, 13.972 in Q2, 14.016 in Q3, and 12.218 in Q4). When we decompose book value into assets and liabilities in the second model, the coefficient on assets is positive (0.913) and the coefficient on liabilities is negative (−0.915).

In the third model, we add the control variables and further decompose total liabilities into warranty liabilities and other liabilities. If the stock market recognizes the dual role of warranty liabilities—contingent liabilities and (positive) information signal—then we expect them to be valued less negatively than other liabilities. That is, we expect $\beta_3 < \beta_2 < 0$. We find the estimated coefficient of −0.605 on *WLIAB* is negative but insignificant at conventional levels ($t = -1.59$; $p = 0.115$). However, this coefficient is less negative than the coefficient on other liabilities, which is negative and statistically significant, rejecting H1a. Thus, the results are consistent with warranty liabilities containing an additional component beyond that of a simple liability. Furthermore, the coefficient on *WLIAB* is higher than that on *OTHER_LIAB*. We perform several additional analyses to shed light on this finding.

First, we include analysts' growth forecasts (*ANALYST_GR*) as an additional explanatory variable under the assumption that future earnings growth is positively correlated with product quality, implying that warranty liability might be priced less negatively than other liabilities. If the stock market correctly values the underlying liability part, then we expect warranty liabilities and

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[19] This is consistent with prior research on valuation of pension liabilities (Landsman 1986; Barth 1991), liabilities of retirees' health benefits (Mittelstaedt and Warshawsky 1993), and environmental liabilities (Barth and McNichols 1994).

[20] We also deflated our variables by total assets. The results are similar to those reported in the study.

[21] In all of our regressions we base our inferences on two-tailed tests and standard errors that are clustered on both firm and fiscal period (Petersen 2009; Gow et al. 2010) to account for potential dependence across multiple observations in the panel. In addition, we winsorize the main variables at the top and bottom 1 percent.

TABLE 4

Market Valuation of Warranty Liability

Panel A: Full Sample

		Dependent Variable = $PRICE_t$							
		Coeff.	**Robust t-stat**	**Coeff.**	**Robust t-stat**	**Coeff.**	**Robust t-stat**	**Coeff.**	**Robust t-stat**
BV_t		1.173***	9.65						
$ASSET_t$	β_1			0.913***	7.35	0.945***	6.08	0.857***	6.13
$LIAB_t$				−0.915***	−4.66				
$WLIAB_t$	β_2					−0.605	−1.59	−1.014***	−2.90
$OTHER\_LIAB_t$	β_3					−0.865***	−4.66	−0.788***	−4.66
$ANALYST\_GR_t$	β_4							0.146***	2.95
$NI\ BEFORE\ WEXP_t$						16.504***	9.97	16.452***	9.83
$WEXP_t$						−16.407***	−9.86	−16.350***	−9.72
NI_t	β_5	12.218***	12.90	12.404***	12.08				
NI_Q1_t	β_6	3.010***	4.17	3.296***	4.96	8.360***	6.44	8.344***	6.46
NI_Q2_t	β_7	1.754**	2.58	1.835***	3.15	0.728	0.48	0.726	0.48
NI_Q3_t	β_8	1.798***	2.89	2.072***	5.07	3.013***	4.38	3.032***	4.38
BM_t						−20.055***	−7.08	−20.061***	−7.78
$LIFE_t$						2.469***	12.17	2.153***	8.79
AGE_t						1.132	1.38	0.872	1.04
Tests of									
$WLIAB_t = OTHER\_LIAB_t$						F = 5.49**	p = 0.02	F = 1.73	p = 0.19
$WLIAB_t = -1$						F = 10.03***	$p < 0.01$	F = 1.18	p = 0.28
Adj. R^2		85.8%		85.8%		90.1%		92.1%	
n		5,868		5,868		4,877		4,877	

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Panel B: Subsample with *TERM* information

		Dependent Variable = $PRICE_t$			
		Coeff.	**Robust t-stat**	**Coeff.**	**Robust t-stat**
$ASSET_t$	β_1	1.150***	9.39	1.175***	9.51
$WLIAB_t$	β_2	−1.169**	−2.55	−1.200***	−3.53
$OTHER\_LIAB_t$	β_3	−1.011***	−8.77	−1.023***	−9.39
$ANALYST\_GR_t$	β_4			0.210***	8.09
SQRT_TERM	β_5	2.342***	3.23	1.440**	2.59
$NI\ BEFORE\ WEXP_t$	β_6	18.667***	15.08	18.636***	15.18
WEXPt	β_7	−18.467**	−14.84	−18.442***	−14.94
NI_Q1_t	β_8	2.264	1.39	2.550	1.61
NI_Q2_t	β_9	1.094	0.70	1.508	0.99
NI_Q3_t	β_{10}	0.636	0.49	0.793	0.62
BM_t		−26.520***	−10.58	−25.819***	−10.50
$LIFE_t$		1.533***	8.77	1.303***	7.55
AGE_t		0.582	1.23	0.847*	1.84
Tests of					
$WLIAB_t = OTHER\_LIAB_t$		F = 0.57	p = 0.45	F = 0.49	p = 0.48
$WLIAB_t = -1$		F = 0.19	p = 0.66	F = 0.28	p = 0.60
Adj. R^2		90.4%		90.8%	
n		1,494		1,494	

Panel C: Subsample with *TERM* Information (Alternative Specification)

		Coeff.	**Robust t-stat**
$ASSET_t$	β_1	1.092***	10.56
$WLIAB_t$	β_2	−1.807***	−7.36
$WLIAB_t * TERM\_MEDIUM$	β_3	0.774***	4.71
$WLIAB_t * TERM\_HIGH$	β_4	1.379***	6.66
$OTHER\_LIAB_t$	β_5	−1.005***	−8.20

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Panel C: Subsample with *TERM* Information (Alternative Specification)

		Coeff.	Robust t-stat
$NI\ BEFORE\ WEXP_t$	β_6	14.585***	10.12
$WEXP_t$	β_7	−13.937***	−9.01
NI_Q1_t	β_8	2.013	1.27
NI_Q2_t	β_9	−0.575	−0.38
NI_Q3_t	β_{10}	−1.114	−1.09
BM_t		−24.881***	−10.53
$LIFE_t$		1.504***	10.84
AGE_t		1.254***	2.70
$TERM\_MEDIUM$		1.198***	3.66
$TERM\_HIGH$		5.937***	6.01
Tests of			
$WLIAB_t = OTHER\_LIAB_t$		F = 6.09***	$p = 0.01$
$WLIAB_t * (1 + TERM\_MEDIUM) = OTHER\_LIAB_t$		F = 0.29	$p = 0.59$
$WLIAB_t * (1 + TERM\_HIGH) = OTHER\_LIAB_t$		F = 5.88**	$p = 0.02$
$WLIAB_t = -1$		F = 5.54**	$p = 0.02$
$WLIAB_t * (1 + TERM\_MEDIUM) = -1$		F = 0.83	$p = 0.36$
$WLIAB_t * (1 + TERM\_HIGH) = -1$		F = 3.96**	$p = 0.05$
$WLIAB_t < 0$		F = 20.55***	$p < 0.01$
$WLIAB_t * (1 + TERM\_MEDIUM) < 0$		F = 12.38***	$p < 0.01$
$WLIAB_t * (1 + TERM\_HIGH) < 0$		F = 7.89***	$p < 0.01$
$WLIAB_t * TERM\_MEDIUM = WLIAB_t * TERM\_HIGH$		F = 4.08**	$P = 0.04$
Adj. R^2		94.9%	
n		1,494	

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*, **, *** Signify statistical significance of two-tailed tests at 10 percent, 5 percent, and 1 percent, respectively.

All panels of Table 4 examine the market valuation of warranty liabilities. The dependent variable is price per share. Coefficients on industry (two-digit SIC code) and quarterly indicators are not shown. Robust t-statistics are based on standard errors that are clustered by both firm and quarter.

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Variable Definitions:

BV = book value per share;
ASSET = total assets per share;
LIAB = total liabilities per share;
WLIAB = warranty liabilities per share;
OTHER_LIAB = total liabilities excluding the warranty liability per share;
NI = earnings before extraordinary items per share;
SQRT_TERM = square root of *TERM* where *TERM* is defined as 1 if the warranty duration is below industry median, 2 if it equals industry median, and 3 if it is above industry median, where industry is defined at the four-digit SIC level with at least ten firms in each industry;
ANALYST_GR = analyst long-term earnings growth forecasts as reported in I/B/E/S; *Q1, Q2, Q3* are indicators for fiscal quarter 1, 2, and 3, respectively;
BM = book-to-market ratio;
AGE = age of the firm computed as the difference between the current year and the year in which the business was originally formed;
LIFE = stage in the firm life cycle defined as a composite index based on yearly rankings of the following variables: (1) median annual dividend as a percentage of income for the prior five years, (2) median annual percent sales growth for the prior five years, (3) median capital expenditure as a percentage of total market value of the firm, and (4) age of the firm (Anthony and Ramesh 1992);
TERM_MEDIUM = indicator variable with the value of 1 when the firm's warranty coverage equals to industry median, and 0 otherwise; and
TERM_HIGH = indicator variable with the value of 1 when the firm's warranty coverage is greater than industry median, and 0 otherwise.

other liabilities to be valued similarly after controlling for growth. The results in Table 4, Panel A indicate that *ANALYST_GR* is positively related to equity prices ($t = 2.95$; $p < 0.01$). Moreover, by including this variable, the coefficient of -1.014 on *WLIAB* becomes significantly negative and close to -1 ($t = -2.90$; $p < 0.01$). An F-test does not reject the hypothesis that the coefficient on *WLIAB* is equal to that on other liabilities ($p = 0.19$). A second F-test does not reject the hypothesis that the coefficient on *WLIAB* is equal to -1 ($p = 0.28$). Combined, these findings are consistent with the market recognizing the signaling component of warranty liabilities, as captured by analysts' growth forecasts.

Second, to further investigate the information role of warranty liabilities, we examine the relation between product quality and warranty liabilities, controlling for the direct signal used by firms. We add to our analysis the terms of warranties' coverages issued by the sample firms as reflected by the warranties' durations (*TERM*). We define *TERM* to equal: (i) 1, if a firm's warranty duration is lower than the industry's median, (ii) 2, if a firm's warranty duration is equal to the industry's median, and (iii) 3, if a firm's warranty duration is higher than the industry's median. We define *TERM* as a relative variable because durations of warranty policies are related to the nature of the products and the industry. Therefore, in a cross-sectional test the relative duration of warranty policies is more informative than their absolute number. We estimate the following model on a subset of firms and report results in Table 4, Panel B:[22]

$$\begin{aligned} P_{i,t} = {} & \beta_0 + \beta_1 ASSET_{i,t} + \beta_2 WLIAB_{i,t} + \beta_3 OTHER\_LIAB_{i,t} + \beta_4 ANALYST\_GR_{i,t} \\ & + \beta_5 SQRT\_TERM + \beta_6 NI\_BEFORE\_WEXP + \beta_7 WEXP + \beta_8 NI_{i,t}{*}Q_1 + \beta_9 NI_{i,t}{*}Q_2 \\ & + \beta_{10} NI_{i,t}{*}Q_3 + Controls + \varepsilon_{i,t}. \end{aligned} \tag{2}$$

In the first model, we introduce *SQRT_TERM*, the square root of *TERM*, to control for warranties' duration, allowing for nonlinearities in this relation.[23] The results show that *SQRT_TERM* is positively related to market values ($t = 3.23$; $p < 0.01$). That is, firms that issue longer-term warranties garner a higher stock price. The square-root specification assumes that the marginal benefit of longer-term warranty diminishes. The coefficient of -1.169 on *WLIAB* ($t = -2.55$; $p = 0.01$) is not statistically different from -1 ($p = 0.66$). In the second model of Panel B, we include *SQRT_TERM*, and *ANALYST_GR*. As in the previous model, the coefficient on *WLIAB* is not statistically different from -1. *SQRT_TERM* remains significant and *ANALYST_GR* is positive and significant ($t = 8.09$; $p < 0.01$). This is consistent with *TERM* serving as a harbinger of the portions of future higher growth that are not captured by analysts' forecasts.

Third, we control for *TERM* in an alternative way to test H1b. This specification allows us to estimate the incremental valuation effect of warranty policies both across as well as within warranty pools. We define three pools and use *TERM_MEDIUM* and *TERM_HIGH* as indicator variables that equal 1 if the warranty terms are equal to or higher than the industry median. We then interact these variables with *WLIAB*. We estimate the following model:

$$\begin{aligned} P_{i,t} = {} & \beta_0 + \beta_1 ASSET_{i,t} + \beta_2 WLIAB_{i,t} + \beta_3 {*} WLIAB_{i,t}{*}TERM\_MEDIUM_{i,t} \\ & + \beta_4 {*} WLIAB_{i,t}{*}TERM\_HIGH_{i,t} + \beta_5 OTHER\_LIAB_{i,t} + \beta_6 NI\ BEFORE\ WEXP_{i,t} \\ & + \beta_7 WEXP_{i,t} + \beta_8 NI_{i,t}{*}Q_1 + \beta_9 NI_{i,t}{*}Q_2 + \beta_{10} NI_{i,t}{*}Q_3 + Controls + \varepsilon_{i,t}. \end{aligned} \tag{3}$$

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[22] In untabulated results we estimate all models in Table 4, Panel A on the subsample of firms with *TERM* information. The inferences are very similar to those in Panel A.

[23] A square root transformation is a special case of the Box-Cox family of transformations. Because the values assigned to the variable *TERM* are essentially rankings, square roots represent an increasing and mildly concave function. We obtain similar results with several other specifications.

Based on H1b, we expect the coefficients to be negative within each pool, but to increase across pools: i.e., $\beta_2 < 0, \beta_2 + \beta_3 < 0, \beta_2 + \beta_4 < 0$ (within each pool) and $\beta_2 < \beta_2 + \beta_3 < \beta_2 + \beta_4$ (across pools). The results in Table 4, Panel C are consistent with H1b. First, they show that within each pool, *WLIAB* and firm value are negatively related. The coefficient of −1.807 on *WLIAB* of the lowest pool (β_2) is negative and significant (t = −7.36; p < 0.01). For the *MEDIUM* pool, the coefficient ($\beta_2 + \beta_3$) is −1.033 and for the *HIGH* pool ($\beta_2 + \beta_4$), it is −0.428; both are significantly different from zero (p < 0.01).[24] Second, across pools, we observe that the coefficients are positive and increasing. That is, β_3 is 0.774 and significant (t = 4.71; p < 0.01) indicating that $\beta_2 < \beta_2 + \beta_3$, and β_4 is 1.379 and significant (t = 6.66; p < 0.01). Third, β_4 is larger than β_3, as indicated by the last F-test reported in Panel C (F = 4.08; p = 0.04). Fourth, the coefficient on *WLIAB*, β_2, which represents the coefficient for the lowest warranty pool is significantly lower than −1, suggesting that these firms are penalized by the market. Finally, consistent with the model in Equation (2), there is some evidence that the marginal benefit of offering a higher level of warranties declines (0.774>1.379 – 0.774 = 0.605; F-stat = 3.45, p = 0.06).

Association of Warranty Information with Future Performance

Another way to shed light on the relation between warranty liabilities and equity values is to examine whether specific components of warranty expenses, in particular abnormal warranty expenses, serve as an indicator of future firm performance. An observed positive relation with future performance could explain the association between warranty liabilities and market values, which we document in Table 4. Specifically, to test H2, we estimate the following model:

$$\Delta ROA_{i,t+\tau} = \beta_0 + \beta_1 ABWEXP_{i,t} + \beta_2 ABCLAIM_{i,t} + \beta_3 ABGM_{i,t} + \beta_4 SALES\_GR_{i,t} + \beta_5 \Delta ROA_{i,t} + \beta_6 SIZE_{i,t} + \beta_7 BM_{i,t} + \varepsilon_{i,t}, \tag{4}$$

where $\tau = 1,2$.[25] We define *ROA* as earnings before warranty expenses and extraordinary items, to avoid any mechanical relation between warranty expenses and future ROA. To ensure that our estimation is robust to the model chosen, we perform the analysis using both the time-series and the two industry models. We include additional controls as follows. Abnormal warranty claims (*ABCLAIM*) controls for changes in product quality. We expect it to have a negative coefficient since higher claim costs are likely to lead to poor future firm performance (Nagar and Rajan 2001). Abnormal gross margin (*ABGM*) may also control for product quality as firms providing high-quality products are able to extract higher margins from their customers. We expect a positive coefficient on this variable because high-quality firms are generally more profitable. We expect both current sales growth (*SALES_GR*) and current change in ROA (ΔROA) to be positively related to the dependent variables because these variables persist in the short run. We expect the coefficient on *BM* to be negative because it is negatively correlated with growth opportunities. We do not make any prediction on the signs of *SIZE*.

Table 5 reports the results of estimating model (4) across pools of firms, providing a test of H2. The first and fourth columns present results using the time-series-based measures of abnormal warranty expenses ($ABWEXP\_{TIME}$) and abnormal claims ($ABCLAIM\_{TIME}$) as independent variables. We find that abnormal warranty expenses are positively associated with changes in ROA in

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[24] We also estimated Equation (3) for each industry at the two-digit SIC code level, using warranty duration to define each pool. The results are qualitatively identical to the results reported in Table 4, Panel C.

[25] We also estimate the model with sales growth, *SALES_GR*, as the dependent variable. The tenor of the results remains unchanged. In addition, when we include as dependent variables the accounting-based metrics in quarter $t+3$, results are similar to those reported for quarter $t+2$.

TABLE 5
Future Performance and Abnormal Warranty Expense

Dependent Variables

	ΔROA_{t+1}			ΔROA_{t+2}		
	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*
	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)
Intercept	0.041	−0.744*	−0.446**	0.304	−0.980*	−0.338
	(0.09)	(−1.80)	(−2.40)	(1.33)	(−1.68)	(−1.49)
$ABWEXP_t$	0.734***	0.372***	0.383***	0.701*	0.189*	0.264*
	(2.77)	(3.02)	(2.80)	(1.77)	(1.96)	(1.87)
$ABCLAIM_t$	−0.938***	−0.290*	−0.168	−1.094***	−0.083	−0.083
	(−4.37)	(−1.74)	(−1.25)	(−2.88)	(−0.79)	(−0.59)
$ABGM_t$	0.327***	0.002	0.001*	0.128**	0.003	0.000
	(2.75)	(0.83)	(1.86)	(2.10)	(1.39)	(0.64)
$SALES\_GR_t$	0.017***	0.013***	0.015***	0.009**	0.009***	0.012*
	(3.32)	(4.34)	(3.60)	(2.62)	(3.35)	(1.87)
ΔROA_t	0.231***	0.628***	0.537***	0.132***	0.541***	0.595***
	(4.03)	(13.69)	(7.18)	(4.65)	(10.36)	(6.19)
STD	−0.862			−0.947		
$(OI/SALES)_t$	(−0.13)			(−0.17)		
$SIZE_t$	−0.002	0.239***	0.400***	−0.009	0.280***	0.533***
	(−0.05)	(4.79)	(3.02)	(−0.08)	(4.15)	(3.09)
BM_t	−1.271***	−1.685***	−1.861**	−0.702***	−1.692***	−1.132
	(−2.69)	(−4.45)	(−2.32)	(−2.77)	(−4.05)	(−1.03)
Adj. R^2	12.0%	34.7%	27.2%	5.5%	24.5%	23.8%
n	3,974	4,494	1,568	3,476	4,029	1,388

*, **, *** Signify statistical significance of two-tailed tests at 10 percent, 5 percent, and 1 percent, respectively.

ΔROA, *SALES_GR*, *ABWEXP*, *ABCLAIM*, and *ABGM* are expressed in percentages. In the industry model, all variables are measured as the deviation from the industry average of other firms where the industry is defined at the two-digit SIC level with at least ten firms in each industry. The robust t-statistics are based on standard errors that are clustered by both firm and quarter. Coefficients on industry and quarterly indicators are not shown.

Variable Definitions:

ROA = defined as earnings before extraordinary items and warranty expenses deflated by beginning-of-year total assets; and

STD (*OI/SALE*) = standard deviation of operating income deflated by sales for the past 8 quarters.

the next quarter ($t = 2.77$; $p < 0.01$) and marginally significant in quarter $t+2$ ($t = 1.77$; $p = 0.08$). This positive relation is consistent with H2 and the information role of warranties across pools of firms. The sign on *ABCLAIM*, which tracks changes in product quality, is negative and significant (coef. = -0.938, $t = -4.37$; $p < 0.01$). This finding is consistent with the ability of changes in product quality, as reflected in abnormal claims, to predict future firm performance

(Nagar and Rajan 2001). The results for the industry models are similar in terms of the sign, but differ in the magnitude of the coefficients because of the different deflator used for the time-series model (total assets) compared to the industry model (sales).

Overall, we conclude that abnormal warranty expenses contain information that is positively related to future firm performance. This relation, in turn, is consistent with warranty liabilities' positive association with market values as documented in the previous section. The source of information in abnormal expenses could be a result of managers applying "informative" discretion to reported earnings by, for example, altering assumptions about expected future failure rates (e.g., Watts and Zimmerman 1986; Bernard and Skinner 1996; Subramanyam 1996).

The cross-sectional analysis in Table 5 covers many firms with different warranty terms across several industries. Thus, it provides evidence on the overall association of abnormal warranty expenses and future performance (H2). However, it is also interesting to separately investigate the association between abnormal warranty expenses and future performance in groups of firms that are in the same warranty pool. In this setting, we expect warranty expenses and future performance to be negatively related, as predicted in H2. In Table 6, we estimate the same model as in Table 5 in four different pools, with each pool consisting of firms in the same industry with the same warranty term. Since this analysis requires a sufficient number of observations in the same industry and warranty pool, we estimate it in four industries. Our focus is on the coefficient on *ABWEXP*. When the dependent variable is changes in ROA in the next quarter, the coefficient is negative and significant in three of the four pools. The results are slightly weaker when we use the changes in ROA in quarter *t*+2. Overall, the analysis provides some support for H2: warranty expenses and future performance are negatively related within pools.

In sum, these analyses demonstrate that warranty liabilities serve two roles, first as a contingent liability and second as an information signal. We show that without controlling for their information role, it is not possible to assess the relation between warranty liabilities and firm value accurately. Further, we show a positive overall association between future firm performance and both warranty liabilities and abnormal warranty expense. This relation, however, is negative in groups of firms that provide identical warranty terms.

Benchmark Beating and Warranty Expenses

In this section, we test our third hypothesis regarding the relation between abnormal warranty expenses and short-term incentives to meet or beat financial reporting benchmarks. We estimate the following regression model:

$$\begin{aligned} Y_{i,t} = {} & \beta_0 + \beta_1 SUSPECT_{i,t} + \beta_2 ABCLAIM_{i,t} + \beta_3 ABGM_{i,t} + \beta_4 BENCHMARK_{i,t} \\ & + \beta_5 SIZE_{i,t} + \beta_6 BM_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{5}$$

The dependent variable, *Y*, is abnormal warranty expenses based on either the time-series or the two industry models.[26] The explanatory variable of interest is *SUSPECT*, defined in three alternative ways: (1) *SUSPECT_ΔNI* takes the value of 1 if the change in pre-managed net income is negative and the change in reported net income is positive, where pre-managed net income is defined as net income before abnormal warranty expense; (2) *SUSPECT_NI* takes the value of 1 if pre-managed net income is negative and reported net income is positive; (3) *SUSPECT_MEET* takes the value of 1 if pre-managed earnings per share misses the last outstanding analyst consensus forecast prior to the quarterly earnings announcement, while the reported earnings per share

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[26] It is important to note that the dependent variable, abnormal warranty expenses (*ABWEXP*), contains some measurement error. However, because we do not believe that there is a correlation between the measurement error and our independent variables, the reported results are not biased. Instead, our model will experience a reduction in explanatory power.

TABLE 6

Future Performance and Abnormal Warranty Expense
(subsample of firms with homogenous warranty plans in different industries)

	Pool 1		Pool 2		Pool 3		Pool 4	
	ΔROA_{t+1}	ΔROA_{t+2}	ΔROA_{t+1}	ΔROA_{t+2}	ΔROA_{t+1}	ΔROA_{t+2}	ΔROA_{t+1}	ΔROA_{t+2}
	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)	Coefficient (Robust t-statistic)
Intercept	1.099*	−1.381	1.220	1.441**	−0.420	1.093***	−0.522	0.841**
	(1.69)	(−0.60)	(0.59)	(2.25)	(−1.50)	(3.88)	(−0.59)	(2.06)
$ABWEXP_t$	−2.590***	−0.231	−1.335***	−0.742	−0.359*	−0.218	−0.892**	−0.609*
	(−3.36)	(−0.22)	(−2.90)	(−1.53)	(−1.69)	(−0.92)	(−2.24)	(−1.77)
$ABCLAIM_t$	−0.785**	−0.035	−3.226***	−0.512	−0.496*	−0.247	−0.633**	−0.383
	(−2.38)	(−0.09)	(−3.04)	(−1.49)	(−1.93)	(−0.57)	(−2.26)	(−0.71)
$ABGM_t$	0.301***	0.054	0.428*	0.059*	0.069***	0.099***	0.072	0.034
	(3.19)	(0.35)	(1.67)	(1.74)	(3.53)	(3.76)	(1.12)	(0.53)
$SALES\_GR_t$	0.026**	0.029**	0.055***	0.007	0.012	0.009	0.074**	0.004
	(2.66)	(2.17)	(3.73)	(0.39)	(1.07)	(0.81)	(2.45)	(0.37)
ΔROA_t	0.955***	1.042***	0.351	0.427**	0.674***	0.022	0.329	0.127*
	(4.06)	(3.38)	(0.72)	(2.00)	(3.78)	(0.46)	(1.23)	(1.66)
$SIZE_t$	1.152**	0.031	3.104**	0.047	0.254	0.422**	0.117	0.058
	(2.07)	(0.05)	(2.62)	(0.26)	(0.56)	(2.18)	(0.36)	(0.30)
BM_t	−3.612**	−0.977	−1.600	−1.856	−0.140	−0.081	−2.434	−3.163***
	(−2.21)	(−1.42)	(−1.28)	(−0.71)	(−0.09)	(−0.08)	(−1.14)	(−4.03)
Adj. R^2	68.7%	53.7%	59.6%	48.4%	73.0%	44.4%	70.8%	58.6%
n	100	85	93	79	71	54	61	44

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*, **, *** Signify statistical significance of two-tailed tests at 10 percent, 5 percent, and 1 percent, respectively.

(continued on next page)

TABLE 6 (continued)

All variables are measured as the deviation from the average of other firms in the same pool. A pool is defined as a group of firms with the same four-digit SIC code and the same warranty duration. Pools with less than ten firms are removed from the sample. Pool 1 includes firms with four-digit SIC code of 3845 and warranty duration of 1 year. Pool 2 includes firms with four-digit SIC code of 3826 and warranty duration of 1 year. Pool 3 includes firms with four-digit SIC code of 3663 and warranty duration of 1 year. Pool 4 includes firms with four-digit SIC code of 3559 and warranty duration of 1 year.

meets or beats analyst consensus forecast, where pre-managed earnings per share is defined as earnings per share before abnormal warranty expense per share.[27] The other explanatory variables (*ABCLAIM* and *ABGM*) are adjusted based on either the time-series or industry models, corresponding to the adjustment of the dependent variable.

Table 7 reports the results. Under all specifications, we find strong evidence of unusually low abnormal warranty expenses in the three samples of firms that are suspected to have managed earnings to achieve benchmarks. All of the coefficients on *SUSPECT_ΔNI, SUSPECT_NI,* and *SUSPECT_MEET* are negative and statistically significant at conventional levels. Specifically, firms reporting an increase in reported net income have lower abnormal warranty expenses, as reflected in the statistically significant negative coefficient on *SUSPECT_ΔNI*, ranging from -0.173 ($t = -12.28$; $p < 0.01$) to -0.483 ($t = -6.27$; $p < 0.01$). This indicates that firms that are suspected to have engaged in opportunistic earnings management reduce warranty expenses significantly more than other firms. Also, the coefficients on *SUSPECT_NI* are negative and significant at the $p < 0.01$ level (ranging from -0.216, $t = -12.40$ to -0.918, $t = -8.02$). Finally, the coefficients on *SUSPECT_MEET* are significantly negative at the $p < 0.01$ level, ranging from -0.152 ($t = -15.88$) to -0.831 ($t = -9.58$).

The results in Table 7 also show that not all of the abnormal warranty expenses are attributable to earnings management. The consistently positive coefficient on *ABCLAIM* in all three benchmark specifications (both in the time-series and in the industry-adjusted model) suggests that as the amount of claims increases, firms allocate more warranty expenses. Overall, the results are consistent with managers using the flexibility in assumptions underlying the warranty expense calculation and exercising their discretion to achieve financial reporting benchmarks.

Valuation of Warranty Liability Combining Growth Expectation and Earnings Management Incentives

Finally, we investigate the market valuation of warranty liabilities by incorporating their contingent liability element, their information signaling role, and short-term earnings management incentives. We use an extension of model (1) as follows:

$$\begin{aligned} P_{i,t} = {} & \beta_0 + \beta_1 ASSET_{i,t} + \beta_2 WLIAB_{i,t} + \beta_3 OTHER\_LIAB_{i,t} + \beta_4 SUSPECT_{i,t} * WLIAB_{i,t} \\ & + \beta_5 SUSPECT_{i,t} + \beta_6 ANALYST\_GR_{i,t} * WLIAB_{i,t} + \beta_7 ANALYST\_GR_{i,t} + \beta_8 NI_{i,t} \\ & + \beta_9 NI_{i,t} * Q_1 + \beta_{10} NI_{i,t} * Q_2 + \beta_{11} NI_{i,t} * Q_3 + \varepsilon_{i,t}. \end{aligned} \tag{6}$$

As documented in the previous subsection, firms with strong incentives to meet or beat earnings benchmarks cut warranty expenses. We identify suspect firms as in Table 7. If investors correctly infer that these firms understate their warranty liabilities, then they would place a larger negative coefficient on warranty liabilities to correct for the underestimation.

Table 8 reports the results of model (6). In the first model, we find that the stock market places a more negative coefficient on warranty liabilities of firms that are suspected to have managed earnings to avoid reporting an earnings decline. The coefficient on the interaction term *SUSPECT* * *WLIAB* is -1.268 with a t-statistic of -2.67 ($p < 0.01$). In the next columns, we find similar results for suspect firms that seek to avoid a loss (coef. $= -1.832$; $t = -2.82$; $p < 0.01$), and those that seek to meet analysts' forecasts (coef. $= -0.606$; $t = -2.86$; $p < 0.01$).

Our measure of analysts' earnings growth expectations (*ANALYST_GR*) is positively associ-

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[27] We also performed analysis using an alternative definition of *SUSPECT*, similar to Roychowdhury (2006). Under this definition, *SUSPECT* is defined based on the proximity of the reported accounting number to the desired benchmark. The tenor of the results is similar to that of the reported results.

TABLE 7
Incentives, Earnings Management and Warranty Expenses

	Dependent Variables = $ABWEXP_t$								
	Avoid Earnings Decline			Avoid Loss			Meet Analyst Forecast		
	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*
	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)
Intercept	0.028	0.011	0.003	−0.073	0.449***	0.250***	−0.021	0.363***	0.183***
	(1.35)	(0.27)	(0.05)	(−1.04)	(6.70)	(6.35)	(−1.36)	(6.67)	(2.89)
$SUSPECT\_\Delta NI_t$	−0.173***	−0.373***	−0.483***						
	(−12.28)	(−4.60)	(−6.27)						
$SUSPECT\_NI_t$				−0.216***	−0.918***	−0.679***			
				(−12.40)	(−8.02)	(−9.17)			
$SUSPECT\_MEET_t$							−0.152***	−0.831***	−0.551***
							(−15.88)	(−9.58)	(−6.86)
$ABCLAIM_t$	0.512***	0.575***	0.809***	0.446***	0.454***	0.706***	0.475***	0.489***	0.702***
	(12.79)	(6.11)	(17.66)	(9.71)	(5.57)	(15.21)	(10.37)	(7.85)	(14.87)
$ABGM_t$	0.249	0.064**	−0.072	−1.151	0.009	−0.065	−0.425	0.222	−0.423**
	(1.09)	(2.20)	(−1.42)	(−1.53)	(0.71)	(−1.50)	(−1.44)	(1.48)	(−2.23)
ΔNI_t	0.258**	5.186	3.411***						
	(2.15)	(0.93)	(4.35)						
NI_t				1.097**	2.689***	6.092***			
				(2.60)	(3.40)	(7.00)			
EPS_t							0.014**	2.163***	0.445***
							(2.50)	(2.85)	(3.90)

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TABLE 7 (continued)

Dependent Variables = *ABWEXP*$_t$

	Avoid Earnings Decline			Avoid Loss			Meet Analyst Forecast		
	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*	Time-Series Model	Industry Model	Industry Model Controlling for *TERM*
	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)	Coef. (Robust t-stat)
SIZE$_t$	0.008***	0.047	0.007	0.007**	0.028**	0.014	0.008***	0.002	−0.046
	(3.80)	(1.22)	(0.33)	(2.59)	(2.41)	(0.67)	(6.68)	(0.14)	(−1.60)
BM$_t$	0.033**	−0.106	−0.175*	0.065**	0.046	−0.139*	0.027**	0.030	−0.552***
	(2.22)	(−0.77)	(−1.83)	(2.25)	(0.78)	(−1.77)	(2.19)	(0.41)	(−3.21)
Adj. R^2	29.9%	53.0%	72.5%	31.6%	60.0%	71.1%	42.1%	66.9%	77.4%
n	4,948	5,530	1,385	5,361	6,043	1,282	3,698	4,835	1,038

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*, **, *** Signify statistical significance of two-tailed tests at 10 percent, 5 percent, and 1 percent, respectively.

ABWEXP, *ABCLAIM*, and *ABGM* are expressed in percentages and are defined in Table 4. In the industry model, all variables are measured as the deviation from the industry average of other firms where the industry is defined as the two-digit SIC level with at least ten firms in each industry. The robust t-statistics are based on standard errors that are clustered by both firm and quarter. Coefficients on industry and quarterly indicators are not shown.

Variable Definitions:

SUSPECT_ΔNI = 1 if the change in pre-managed net income is negative and the change in net income is positive, where pre-managed net income is defined as net income before abnormal warranty expense;

SUSPECT_NI = 1 if pre-managed net income is negative and net income is positive;

SUSPECT_MEET = 1 if a firm's pre-managed earnings per share misses the last outstanding analyst consensus forecast prior to the quarterly earnings announcement, while the earnings per share meets or beats analyst consensus forecast, where pre-managed earnings per share is defined as earnings per share before abnormal warranty expense;

SIZE = logarithm of the market value of equity at the beginning of the quarter; and

NI = earnings before extraordinary items scaled by lagged total assets.

TABLE 8

Valuation of Warranty Liability Incorporating Growth and Earnings Management (Full Sample)

Dependent Variable = $PRICE_t$

	Avoid Earnings Decline		Avoid Loss		Meet Analyst Forecast	
	Coeff.	**Robust t-statistic**	**Coeff.**	**Robust t-statistic**	**Coeff.**	**Robust t-statistic**
$ASSET_t$	1.093***	13.22	0.944***	9.06	0.992***	9.00
$WLIAB_t$	−0.842***	−3.43	−1.082***	−4.61	−0.890***	−3.37
$OTHER\_LIAB_t$	−0.938***	−7.60	−0.769***	−5.09	−0.806***	−5.13
$SUSPECT_t * WLIAB_t$	−1.268***	−2.67	−1.832***	−2.82	−0.606***	−2.86
$SUSPECT_t$	2.841***	4.66	6.961***	8.50	4.991***	8.11
$ANALYST\_GR_t * WLIAB_t$	0.010***	3.02	0.014**	2.23	0.059***	2.76
$ANALYST\_GR_t$	0.043***	4.93	0.105***	3.93	0.118***	3.31
NI_t	13.047***	10.00	11.417***	7.23	13.450***	7.89
NI_Q1_t	3.170***	6.60	2.883***	4.19	3.119***	4.86
NI_Q2_t	1.717	1.03	1.685*	1.76	1.815*	1.68
NI_Q3_t	2.111***	4.01	1.466***	3.00	1.556***	2.90
Tests of						
$WLIAB_t * [1 + \text{Median}\ (SUSPECT) + \text{Median}\ (ANALYST\_GR_t)] = OTHER\_LIAB_t$						
	F = 0.33	p = 0.56	F = 0.46	p = 0.50	F = 1.23	p=0.27
$WLIAB_t * [1 + \text{Median}\ (SUSPECT) + \text{Median}\ (ANALYST\_GR_t)] = -1$						
	F = 0.10	p = 0.75	F = 0.59	p = 0.44	F = 2.15	p=0.14
Adj. R^2	0.873		0.880		0.894	
n	4,965		4,954		4,659	

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*, **, *** Signify statistical significance of two-tailed tests at 10 percent, 5 percent, and 1 percent, respectively.

This table shows market valuation of warranty liability after incorporating earnings management incentives. The dependent variable is price per share. Coefficients on industry and quarterly indicators are not shown. All the independent variables except $SUSPECT_t$ and $ANALYST\_GR_t$ are deflated by common shares outstanding. The robust t-statistics are based on standard errors that are clustered by both firm and quarter.

Variable Definitions:

SUSPECT = *SUSPECT_ANI* in the "avoid earnings decline" regression; *SUSPECT_NI* = "avoid loss" regression; and *SUSPECT_MEET* = "meet analyst forecast" regression. *SUSPECT_ANI*, *SUSPECT_NI*, and *SUSPECT_MEET* are defined as in Table 7.

ated with share price across all three models. We add an interaction term between *ANALYST_GR* and *WLIAB* to examine whether the information signaling in warranty liabilities varies across firms with different growth opportunities. The coefficient on the interaction term is positive and significant for all three earnings benchmarks, indicating that warranty liabilities serve as a stronger information signal for high-growth firms than for low-growth firms.

More formally, we conduct an F-test of whether the coefficient on *OTHER_LIAB* is equal to the sum of the coefficient of *WLIAB* and its interactions with *SUSPECT* and *ANALYST_GR*, both evaluated at their median values. The results of the test indicate that there is no evidence to reject the hypothesis that the coefficients of *WLIAB* and *OTHER_LIAB* are equal (p-values = 0.56, 0.50, and 0.27). Further, we use another F-test to examine whether the coefficient on *WLIAB* is different from -1. We cannot reject the hypothesis that $WLIAB = -1$ (p-values = 0.75, 0.44, and 0.14). In an unreported analysis of the subsample of firms for which *TERM* is available, our conclusions remain unchanged after controlling for the duration of the warranties, and incorporating duration in the industry-based measures of *ABWEXP* and *ABCLAIM*.

Overall, the results in Table 8 support the conjecture that warranty liabilities represent three aspects: a contingent liability, an information signal about growth prospects, and an earnings management tool. We find that the stock market values warranty liabilities more negatively for firms that have managed earnings and that it places a positive weight on warranty liabilities as a signal of future growth prospects. After controlling for signaling and earnings management, we find that the stock market values warranty liabilities similarly to how it values other recognized liabilities.

VI. CONCLUSION

We use a sample of over 800 firms that disclose warranty information under FIN 45 to investigate the economics and accounting aspects of product warranties. Our study provides insights into the market interpretation of warranty disclosures and managers' choices with regard to product warranty policies as well as the accounting treatment of warranties.

We first investigate the market valuation of warranty liabilities. We hypothesize that they serve as both contingent liabilities that reflect future services related to warranty obligations as well as an information signal of product quality and future growth prospects. Our findings indicate that the stock market places a smaller negative valuation coefficient on warranty liabilities compared to other reported liabilities. When we control for the signaling role of warranty liabilities (with analyst growth expectations and warranty duration), the valuation coefficients on warranty liabilities and other liabilities approach -1. We also show that the relation between market values and warranty liabilities varies across different groups of firms, depending on the level of warranty coverage they provide. The market assigns a higher valuation coefficient on warranty liabilities of firms that provide superior warranty coverage. In addition, we report a positive association between abnormal warranty expenses and future firm performance across firms that provide different warranty coverage. However, within groups of firms that provide identical coverage, this relation is negative. Together, these findings support the hypothesis that the market interprets warranty liabilities as information signals for product quality and future growth prospects.

We also find evidence that firms with incentives to manage earnings to meet earnings targets report lower abnormal warranty expenses. This evidence is consistent with managers using their discretion in the estimates of warranty accruals to achieve financial reporting targets.

Finally, we investigate the market valuation of warranty liabilities after controlling for signaling and earnings management aspects. We show that warranty liabilities reduce share prices dollar-for-dollar. We also document that investors understand that warranty liabilities of firms that engaged in earnings management are understated.

Overall, the findings in this study demonstrate a unique information aspect of warranty liabilities. Our findings also show that disclosures on warranties allow this information to be utilized by market participants.

APPENDIX A
SAMPLE WARRANTY DISCLOSURES

Dell Corp.

	Fiscal Year Ended		
	February 1, 2008	February 2, 2007	February 3, 2006
		(in millions)	
Warranty liability:			
Warranty liability at beginning of year	$ 958	$ 951	$ 722
Costs *accrued* for new warranty contracts and changes in estimates for pre-existing warranties[(a)]	1,141	1,242	1,391
Service obligations honored	(1,170)	(1,235)	(1,162)
Warranty liability at end of year	$ 929	$ 958	$ 951
Current portion	$ 690	$ 768	$ 714
Non-current portion	239	190	237

(a) Changes in cost estimates related to pre-existing warranties are aggregated with accruals for new warranty contracts. Dell's warranty liability process does not differentiate between estimates made for pre-existing warranties and new warranty obligations.

Western Digital

Product Warranty Liability

Changes in the warranty accrual for 2008, 2007 and 2006 were as follows (in millions):

	2008	2007	2006
Warranty accrual, beginning of period	$ 90	$ 89	$ 92
Charges to operations	106	74	76
Utilization	(73)	(52)	(49)
Changes in estimate related to pre-existing warranties	(9)	(21)	(30)
Warranty accrual, end of period	$ 114	$ 90	$ 89

APPENDIX B

This appendix provides a brief discussion on how warranty costs and types are related in a separating equilibrium. We show that warranty costs and types are positively related if the difference in the cost of providing warranty services by type is not too large.[28]

Consider a simple model with two adjacent types: L and H. Let type H be a higher type. Both type H and L know their own type, but potential buyers do not. As the quality is higher, type H's product breaks down less frequently and is less costly to repair. Given the asymmetry of information, type H tries to signal its quality by providing a better warranty plan (w_2). Buyers believe that the provider of warranty plan 2 is the higher type. In a separating equilibrium, type H provides w_2, while type L provides w_1, which is inferior to w_2. For a product with w_2, buyers are willing to buy q_2 units at the price p_2. Type H is then expected to incur $c^H(w_2)q_2$ as the total warranty-servicing cost. Type L offers w_1, sells q_1 units at p_1, and incurs $c^L(w_1)q_1$. Thus, given the beliefs of the buyers, the warranty choices made by both types form a Perfect Bayesian equilibrium. For a separating equilibrium to exist, the higher type must have a cost advantage such that:

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[28] For interested readers, a research note that provides a full discussion is available from the authors upon request.

$$c^L(w_2) - c^L(w_1) > c^H(w_2) - c^H(w_1),\ c^L(w_1) > c^H(w_1),$$

and $c^L(w_2) > c^H(w_2)$. These inequalities are implied by the single-crossing property.

In the separating equilibrium, neither type wants to mimic the other. That is:

$$(p_1 - c^L(w_1))q_1 \geq (p_2 - c^L(w_2))q_2, \tag{B1}$$

$$(p_2 - c^H(w_2))q_2 \geq (p_1 - c^H(w_1))q_1. \tag{B2}$$

Equations (B1) and (B2) are the self-selection constraints for type L and H, respectively. Combining the two equations, we have:

$$c^L(w_2)q_2 - c^L(w_1)q_1 \geq p_2q_2 - p_1q_1 \geq c^H(w_2)q_2 - c^H(w_1)q_1. \tag{B3}$$

Equation (B3) says that the marginal benefit justifies the marginal cost of providing a better warranty plan for type H, but not for type L.

To see how total warranty costs and types are related in equilibrium, we compare $c^L(w_1)q_1$ with $c^H(w_2)q_2$. If they are to be (strictly) positively related, then we must have:

$$c^H(w_2)q_2 > c^L(w_1)q_1 \text{ or } c^H(w_2) > c^L(w_1)\frac{q_1}{q_2}. \tag{B4}$$

Condition (B4) implies that $c^H(w_2)$ cannot be too small relative to $c^L(w_1)$. While the single-crossing property requires that type H has a cost advantage, the cost advantage cannot be large.[29] For example, suppose $q_1 = q_2$ (a better warranty plan commands a higher price $[p_1 < p_2]$ but does not increase the sales volume). Then, Condition (B4) states $c^H(w_2) > c^L(w_1)$, that is, the unit cost of providing a better warranty plan (w_2) by type H is higher than that of providing the inferior plan (w_1) by type L. If $q_1 < q_2$ (a better warranty plan increases the sales volume), then it is possible to have $c^H(w_2) < c^L(w_1)$. However, $c^H(w_2)$ is bounded below.

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[29] To see that the relation can be negative, imagine a firm that produces a perfect product that never fails. The firm can signal its type by providing full warranty coverage but never incurring warranty costs. Other firms without the same technology would incur a positive cost to offer any level of warranty (or they might choose not to offer any warranty plan). The relation in this case is negative (or zero).

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Investment Growth and the Relation between Equity Value, Earnings, and Equity Book Value

Shengquan Hao
Shanghai Jiaotong University

Qinglu Jin
Shanghai University of Finance and Economics

Guochang Zhang
Hong Kong University of Science and Technology

Abstract: Using Zhang (2000) as the theoretical basis, we predict and empirically test the effect of investment growth on the relation between equity value and accounting variables. We find that (1) growth increases the slope in the value-earnings relation for high-profitability firms (consistent with growth having positive NPV), but has an insignificant or negative effect on the slope for lower-profitability firms (consistent with growth having non-positive NPV); (2) given earnings, growth increases the (positive) slope of the relation between equity value and equity book value for low-profitability firms, but reduces this slope and causes equity value to *decrease* with book value for high-profitability firms; and (3) given profitability (ROE), equity value uniformly increases with book value, and growth increases the slope of this relation. We also examine the valuation impact of *past* investment activities that arises from accounting conservatism. We find that the earnings coefficient is greater in the years following faster investment increases (which cause earnings to be more conservatively stated).

Keywords: *investment growth; equity valuation; earnings; equity book value; profitability; conservatism.*

Data Availability: *The data used in this study are available from public sources.*

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I. INTRODUCTION

Growth opportunities constitute an important part of firm value (Miller and Modigliani 1961; Dixit and Pindyck 1994). When investors determine firm value based on reported accounting data, it is essential that they consider how much the firm is expected to grow. But as Holthausen and Watts (2001) observe, empirical research on accounting-based valuation has largely neglected the growth factor. In this study, we reexamine the accounting-value link by explicitly recognizing the role of growth in valuation. Using the real-options-based model of Zhang (2000) as the theoretical basis, we predict and empirically test how the relation between equity value and accounting data behaves in a distinctly nonlinear fashion and, in particular, how investment growth mediates this nonlinear relation.

In this study, growth refers to increases (or decreases) in the amount of capital invested in business operations.[1] Economic reasoning suggests that whether investment growth enhances value generation depends on profitability,[2] defined as earnings in a period divided by the equity book value at the beginning of the period (ROE). Profitability represents a firm's ability to generate value from invested capital, thus indicating the desirability of increasing or reducing the scale of operations. Mirroring its investment-guiding role, profitability thus helps investors to infer the firm's course of operations going forward. Because of its fundamental role in determining investment decisions underlying value generation, profitability (as opposed to earnings) serves as an appropriate valuation metric.[3] Obviously, investment growth undertaken by a profitable firm enhances investor value, whereas that by an unprofitable firm creates no value or even destroys value. Our analysis demonstrates how this basic notion about growth, profitability, and value creation manifests in mapping accounting data to value.

We develop four hypotheses concerning various aspects of the growth effect on the valuation function. We first test these hypotheses separately with partial model specifications (which collapses one of the dimensions of the valuation space), and then jointly with "full" model specifications. In an extended analysis, we further follow Zhang (2000), who predicts a link between the trend of past investment changes and earnings biases, to introduce the effect of conservative accounting. There, we adopt a more comprehensive empirical model within which we test the effects of both conservatism and investment growth.

Existing research shows that, given equity book value, equity value increases with earnings, and growth increases the slope of the value-earnings relation (Collins and Kothari 1989; Kothari 2001). We predict that, whereas growth strengthens the value-earnings relation in terms of a steeper slope for high-profitability firms (whose investment growth will generally have positive NPV), the effect is expected to be smaller and can even disappear for low-profitability firms (whose growth will generally have low or non-positive NPV).[4] Our empirical results are consistent. We first show that in regressions of stock price on earnings controlling for book value, the slope is positive and becomes increasingly steeper, confirming convexity as predicted by real options (Burgstahler and Dichev 1997; Zhang 2000). More importantly, while growth increases the slope of the value-earnings relation in high-profitability regions, it has either an insignificant or a negative effect on the slope in low-profitability regions.

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[1] The growth construct in this study is input (investment) based. This is conceptually distinct from, though related to, (output-based) earnings growth. Whether investment growth translates into earnings growth depends on a firm's profitability. Ohlson and Juettner-Nauroth (2005) provide a model that relates equity value to *expected* earnings and earnings growth.

[2] Fama and Miller (1972) provide an illustration of this basic notion. Bar-Yosef et al. (1987) empirically investigate the relation between investment and earnings.

[3] Note that earnings cannot be compared directly across firms with different scales of operations. The importance of profitability for valuation means that income statement data should be interpreted *in conjunction with* the balance sheet, consistent with the opinion of the Financial Accounting Standards Board (see SFAC No. 5, paragraph 24a).

[4] The rationale is that a firm's strength/weakness in business fundamentals should persist (to some extent) from one period to the next, so that firms with higher profitability currently are expected to have higher profitability in the following periods. Empirical evidence from Dechow et al. (1999) and Biddle et al. (2001) is consistent with the premise of our argument.

According to prior studies (Burgstahler and Dichev 1997; Collins et al. 1999), equity value is positively related to equity book value given earnings, but little is known about how growth intervenes in this relation. We explain that, while a positive relation should generally hold absent growth, investment growth will reduce the slope of this relation in high-profitability regions (where growth options are important); in fact, the negative effect of growth can be so dominant that it causes equity value to *decrease* with equity book value. This negative relation in high-profitability regions caused by growth options is contrary to a predicted positive relation in low-profitability regions caused by adaptation options (Burgstahler and Dichev 1997). Together, the two forces produce an equity value function that changes nonmonotonically with book value over a wide profitability range.

Our results support these predictions. Controlling for earnings, we find a negative coefficient on equity book value in the regions of low book value (corresponding to high profitability); however, the slope turns positive in the regions of high book value (low profitability). After explicitly recognizing the growth effect in regressions, we find that growth reduces the slope coefficient (i.e., makes it more negative) on book value in high-profitability regions, but increases the slope (i.e., makes it more positive) in low-profitability regions.

However, the above-described behavior does not portray a full view of the relation between equity value and equity book value in cross sections. If we hold profitability (instead of earnings) constant, then the relation changes sharply. In this case, equity value increases uniformly with book value, and growth increases the slope of this relation. The intuition is that among firms equally efficient in generating value from invested capital, those with more capital invested will have higher values; this is true especially for firms expected to undertake high growth.

Whereas the above effects of investment growth are induced by *economic* forces for value creation, Zhang (2000) explains that investment activities can also influence the value-accounting relation for *accounting* reasons. Under conservative accounting practices, the speed of increased investment in recent years will determine the degree of conservatism in earnings. Consistent with this prediction, we find that the earnings coefficient is greater in the high- versus low-earnings conservatism group (faster investment increases in the recent past). At the same time, we find that the predicted growth effect holds in both high- and low-earnings-conservatism groups, confirming that the economic-driven effect of investment growth is distinct from the conservatism-driven effect of past investments.

Our study extends the empirical valuation literature by showing how the nonlinear relation between equity value, earnings, and equity book value is dependent on investment growth. Previous research has relied heavily on linear valuation relations,[5] motivated by either heuristic arguments or the models of Ohlson (1995) and Feltham and Ohlson (1995), assuming linear information dynamics. However, empirical evidence generally does not support these linear models (Dechow et al. 1999; Callen and Segal 2005). Some criticize these models as nondescriptive because they do not capture positive economic rents or growth options (Lo and Lys 2000; Biddle et al. 2001; Holthausen and Watts 2001). Burgstahler and Dichev (1997) explicitly examine nonlinearity in valuation, but they focus on the role of adaptation options (relevant to unprofitable firms), not that of growth options (relevant mainly to profitable firms).

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[5] Examples include Landsman (1986), Barth (1991), and Shevlin (1991), who use balance sheet data, Barth et al. (1992), who use income statement data, and Collins et al. (1999), Lev and Zarowin (1999), and Core et al. (2003), who use a linear combination of earnings and equity book value. See Holthausen and Watts (2001) for a survey.

Our study answers the call by Holthausen and Watts (2001) for more valuation research that incorporates rents and growth options. Although several studies have demonstrated the important role of growth options, their focuses differ from ours. Zhang (2000) builds growth options into a theoretical model, but does not conduct a corresponding empirical examination. Biddle et al. (2001) explore the nonlinear dynamic behavior of residual income under the notion of "capital following profitability" (the key assumption giving rise to real options), but do not examine the behavior of equity value. Finally, Chen and Zhang (2007) test a return model derived from the value model of Zhang (2000). But determining equity return over a time period is a distinctly different problem from determining equity value at a given point in time. Returns derive from changes in value (but one cannot determine a firm's value from observed returns), so the valuation question examined in our study is more basic than that in Chen and Zhang (2007). Importantly, unlike Chen and Zhang (2007), who use the growth variable as a separate explanatory factor—along with their accounting variables—in the return model, our study explores how the valuation impact (the slope coefficients) of accounting variables is dependent on growth, thus emphasizing the interactive effects of growth and accounting variables in valuation.

In addition to examining the role of *prospective* investment (growth options), this study contributes to the literature by showing how *past* investment activities affect the value-accounting relation as a result of conservative accounting practices. Finally, beyond testing the hypotheses of this study, the empirical models we estimate also confirm results established from previous studies concerning, for example, convexity (Burgstahler and Dichev 1997), and the relative importance of earnings versus equity book value for positive and negative earnings firms (similar to our high- and low-profitability firms; Collins et al. 1999). Our study thus helps to unify various empirical findings within a common framework.

In Section II, we develop the hypotheses. Section III describes the sample. Section IV uses partial model specifications to test the hypotheses, while Section V uses full model specifications. Section VI tests the implications of conservative accounting. Finally, we describe our robustness checks in Section VII and conclude in Section VIII. The mathematical derivations of the hypotheses are in the Appendix.

II. THEORETICAL FOUNDATION AND HYPOTHESES

In this section, we first use a simplified version of Zhang's (2000) model, which assumes unbiased accounting depreciation, to develop four hypotheses on the valuation effect of investment growth; these hypotheses all arise from *economic* incentives for value generation. We then consider the general version of Zhang's model to develop our fifth hypothesis, which arises from accounting conservatism (as represented by a conservative depreciation policy in Zhang [2000]).

A Simplified Version of Zhang's (2000) Model

Let V be the (current) equity value of an all-equity-financed firm, B the corresponding equity book value, X the current-period earnings, g the extent to which the scale of operations may grow in the future, and k the earnings capitalization factor.[6] Define $q \equiv X/B$ as the firm's (current-period) profitability. Then, following Zhang (2000) and assuming unbiased accounting depreciation, equity value can be expressed as:

$$V = B[P(q) + kq + gC(q)] = BP(q) + kX + gBC(q) \tag{1}$$

where $P(.)$ is the (put) option to abandon operations, and $C(.)$ is the (call) option to grow, both normalized by the scale measure, B. The decisions to exercise the real options are dependent on profitability, making the options functions of q. Panels A and B of Figure 1 provide geometric views of model (1) in the space defined by axes q, B, and V, for two different values of g. The valuation function is represented as a curved surface that maps (q, B) combinations into Vs.[7]

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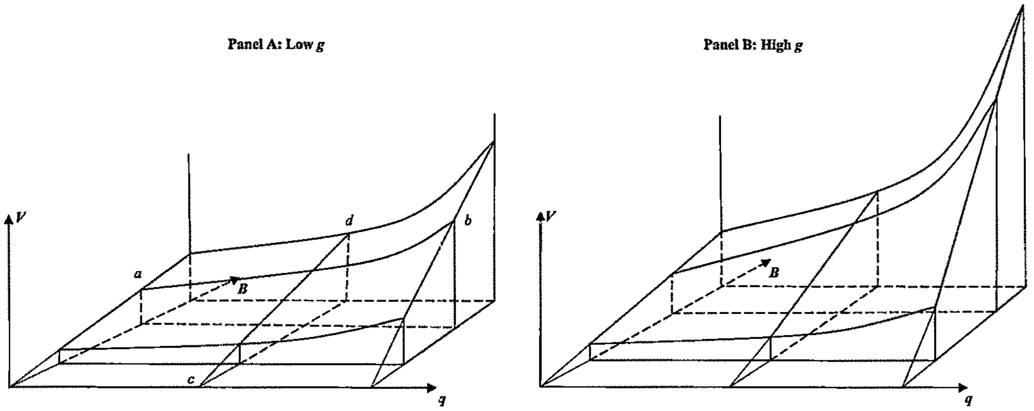
[6] Since we are focusing on the role of *contemporaneous* accounting data, we suppress the time subscripts on variables for simplicity.

Growth and the Relation between Equity Value and Earnings, Given Book Value

We now employ model (1) to explore how equity value changes with earnings at a given equity book value. The basic V-X relation is depicted in Figure 1, Panel A. At a given B, moving along the q-axis, we trace out V as an increasing and convex function of X (see Predictions 1 and 2 of Zhang [2000, 283]), as represented by curve *ab*. In the extremely low (possibly negative) q region, the slope in the V-X relation is close to zero. The reason is that when operations are so unprofitable that they are likely to be discontinued, equity value depends primarily on book value (proxying for the exit or adaptation value), with earnings being of little use in predicting value generation. As X increases (and hence q increases, given B), the probabilities also increase that the firm will stay in business and its operations will grow. This raises expectations about future earnings, thus increasing the slope in the V-X relation.

In model (1), growth parameter g magnifies the value of the growth option. In high-q regions where the option is in the money, the slope of the V-X relation will increase with growth, as illustrated in Panels A and B of Figure 1 for two different values of g. Moving to the lower-q

FIGURE 1
Geometrical Views of Valuation Model (1)



This figure presents geometrical views of equity value (V) as a function of profitability (q) and book value (B) based on model (1) for low growth (Panel A) and high growth (Panel B). At a given B, V is an increasing function of q, as indicated by curve *ab* in Panel A, and the slope increases with q. At a given q, V is an increasing function of B, as indicated by line *cd* in Panel A.

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[7] Axes B and q are orthogonal because their information contents are complementary in nature. Note that earnings are dependent on scale, and thus would be correlated with book value.

regions, the growth option becomes less valuable and, consequently, the incremental slope effect of high (versus low) growth diminishes.

In the extreme low-q region, growth should create little or no value. Why firms in this region still undertake growth is outside the model of Zhang (2000), but this can occur for a number of reasons. One is that these firms have temporarily low earnings, such that current profitability is not representative of future profitability. Another is that managers may undertake growth to advance personal interests at the expense of investors. Thus, how growth influences valuation mapping in low-profitability regions is an open empirical issue; to the extent that growth undertaken by these firms does not create value (or destroys value), the growth effect on the slope will disappear (or turn negative).

The above discussion leads to H1 below (stated in alternative form) about the effect of growth as predicted from Zhang (2000). We present qualitative arguments for the hypotheses in this section, and provide the mathematical derivations in the Appendix.

H1: Given equity book value, growth has a positive effect on the slope of the value-earnings relation in high-profitability regions; this effect becomes smaller in lower-profitability regions.

Growth and the Relation between Equity Value and Book Value, Given Earnings

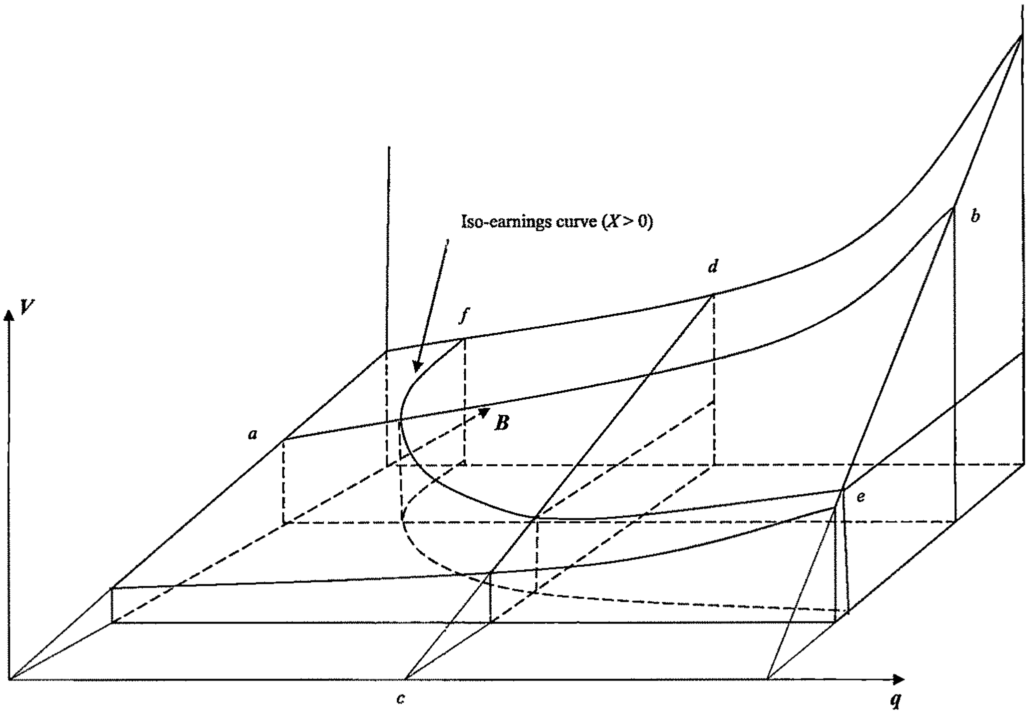
Zhang (2000, 284–285) shows that, given earnings, the relation between equity value and equity book value is nonmonotonic. In the low-q region (where firms have a high B, given X), $P(q)$ is in the money, whereas $C(q)$ is out of the money. Equity value in this case depends primarily on book value, producing a positive relation between V and B. On the other hand, in the high-q region (where firms have a low B), while $P(q)$ is out of the money, $C(q)$ is valuable. Since firms with larger book values are less profitable (holding earnings constant), their growth options are less valuable than those of firms with smaller book values (which are more profitable), causing an inverse relation between V and B in the high-q region. The nonmonotonic relation between V and B, given X, is illustrated by curve *ef* in Figure 2.

Again, growth changes the valuation mapping, with the extent of the growth effect dependent on profitability. As explained above, in the high-q region, the growth option is highly sensitive to profitability, which induces an inverse relation between V and B. Following this reasoning, higher growth will exacerbate the effect of growth options, causing the inverse V-B relation more pronounced. This suggests that growth should have a negative effect on the slope of the V-B relation (i.e., make the slope more negative) in the high-q region.

The situation is different in the low-q region. The scenario of low-profitability firms undertaking growth lies outside Zhang's (2000) model, and how growth influences valuation mapping is an empirical issue. Intuitively, investment growth increases the proportion of newly acquired assets relative to the old assets of a firm. The relevant question then is: In the event of abandonment, how easily are new assets adapted to alternative business uses relative to old assets? To the extent that the former are more adaptable and, thus, have higher exit values than the latter (on a per-unit basis), a higher proportion of new assets will increase the percentage of asset values to be recovered, suggesting that growth will increase the slope of the V-B relation in the low-q region.

The above discussion leads to H2 and H3 below. Note that H2 and part (i) of H3 are based on Zhang (2000), whereas part (ii) of H3 is a prediction outside Zhang's (2000) model.

FIGURE 2
Relation between Equity Value and Equity Book Value, Given Earnings



This figure illustrates that equity value (*V*) is a nonmonotonic function of book value (*B*), given earnings (*X*). Curve *ef* is an iso-earnings curve. As we move along curve *ef*, starting from point e, *V* first decreases with *B* in the high-profitability (low-book-value) region and then increases with *B* in the low-profitability (high-book-value) region.

H2: Given earnings, equity value increases with equity book value in low-profitability (high-book-value) regions, and decreases with book value in high-profitability (low-book-value) regions where growth options are important.[8]

H3: (i) Given earnings, growth reduces the slope of the relation between equity value and book value in high-profitability regions (i.e., makes the slope more negative); and (ii) growth increases this slope in low-profitability regions (i.e., makes the slope more positive).

Growth and the Relation between Equity Value and Book Value, Given Profitability

The relation between equity value and book value can change sharply, however, if we hold profitability constant, rather than earnings. Refer to Figure 1, Panel A. For a given q, moving along axis B we trace out V as an increasing function of B, as represented by the line *cd*. This means that, among firms equally capable of generating profit from invested capital, those with more invested capital have higher values, resulting in a positive *V-B* relation given q. This behavior of the *V-B* relation is distinct from that explained above, where we control for X in the cross-section.

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[8] This nonmonotonic *V-B* relation, given *X*, is in contrast to the prediction of Burgstahler and Dichev (1997, their Proposition 2) that equity value is an increasing function of book value given earnings, arising from a setting with adaptation options but no growth options.

The influence of growth on the *V-B* relation is also different here. *Ceteris paribus*, the operations of firms with more growth opportunities will grow faster than will those of firms with fewer opportunities, which will, in turn, generate more earnings. Thus, growth increases the slope in the *V-B* relation, given q, in the high-q region. In the low-q region, a more relevant notion of value is adaptation value. On the premise that newly invested assets are more adaptable to alternative business uses than are older assets (as discussed above), we conjecture that growth also increases the slope of the *V-B* relation in the low-q region.

We summarize this discussion in H4. Note that part (i) is based on Zhang (2000), whereas part (ii) is an intuitive prediction outside Zhang's (2000) model.

> **H4:** (i) In high-profitability regions, growth increases the (positive) slope in the relation between equity value and equity book value, given profitability; and (ii) in low-profitability regions, growth also increases the slope in this relation.

Past Investment Activities and Conservatism in Earnings

Whereas the above analysis illustrates the valuation effect of *prospective* investment activities that arises from economic incentives for value creation, the general version of Zhang's model (Zhang 2000, 279, Equation 24) further incorporates conservative accounting, giving rise to the role of *past* investments in influencing the valuation function. In Zhang (2000), accounting is based on historical cost, and the source of accounting biases is a conservative policy that recognizes depreciation at a rate *faster* than the actual decline of the productive capacity of assets. Unbiased accounting obtains in the special case in which depreciation is recognized in an unbiased way, commensurate with the actual decline in physical asset stock.[9]

Specifically, under a conservative depreciation policy, firms recognize more expense (relative to unbiased depreciation) in the initial years of an asset's life, but less expense in the latter years when the cumulative over-recognition gets unwound. In a given year, the direction and magnitude of earnings bias depend jointly on the amount of depreciation over-recognition on newly invested assets and the amount of under-recognition on older assets (i.e., the unwinding of previous over-recognition). Following periods of *increases* in capital investments, the depreciation over-recognition on newly invested assets (which are in greater amounts) outweighs the depreciation under-recognition on older assets (which are in smaller amounts), causing the firm's overall earnings to be understated (relative to the case of unbiased depreciation). On the other hand, following periods of *decreases* in investments, the opposite result occurs, whereby the depreciation over-recognition on newly invested assets is outweighed by the unwinding of previous over-recognition on older assets, causing the firm's overall earnings to be overstated.[10]

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[9] The notion of conservatism here is similar to that in Penman and Zhang (2002) and Rajan et al. (2007). With historical cost-based accounting, equity book value relates to realized investments but not to expected future activities. In such a setting, even under unbiased accounting, equity book value on average will be below equity market value owing to (for example) expected positive NPV investments in the future. An alternative and fundamentally different notion of conservatism is proposed by Ohlson (1995) and Feltham and Ohlson (1995), which is in terms of whether equity book value is expected to be below or equal to equity market value. Under this latter notion, unbiased accounting requires that equity book value be marked to market, and consequently income be determined as changes in equity market value; this requires that equity book value incorporate not only past investments, but also the expected value from future investments.

[10] See Zhang (2000, 278–279) for a more detailed analysis. The link between past investment and earnings biases is similarly illustrated by Rajan et al. (2007).

However, investors will correct for accounting biases in valuation. Following the above discussion, we predict an increased valuation weight on earnings following periods of increases in investments (because earnings are understated) and a reduced weight following periods of decreases in investments (because earnings are overstated) (see Zhang 2000, 283, Hypothesis 1). This leads to our fifth hypothesis, which concerns the relation between the trend of past investment changes and the slope of the value-earnings relation.[11]

> **H5:** *Ceteris paribus*, the slope of the value-earnings relation is greater for firms experiencing fast investment increases in recent periods than for firms experiencing slow (or negative) investment increases.

III. SAMPLE AND DESCRIPTIVE STATISTICS

We extract the data for empirical analysis from the Compustat database. The variable definitions and specific data items are as follows: V (market value per share) is the market price of common shares (Compustat data item #199) at the fiscal year-end; B (book value per share) is the book value of equity (#60) divided by the number of common shares outstanding (#25), both at the fiscal year-end; X (earnings per share) is diluted earnings per share excluding extraordinary items (#57);[12] and q (profitability) is earnings before extraordinary items divided by the book value of equity at the beginning of the fiscal year.[13] Growth in Zhang's (2000) model refers to prospective investment growth. Empirically, we measure growth (g) by the average realized annual growth rate of equity book value, adjusted for the Consumer Price Index, over the subsequent three years (similar to the growth measure in Biddle et al. [2001]).[14]

We require firms to have at least four consecutive years of data to be included in the sample (due to using *ex post* growth measures). We delete observations with missing data on earnings per share (X), book value per share (B), market value per share (V), profitability (q), or growth (g). We also remove regulated and financial industries (as in Burgstahler and Dichev 1997). To mitigate the effect of outliers, we exclude firms with a current- or prior-year total equity book value of less than \$1 million, and winsorize all continuous variables in our regressions at the top and bottom 1 percent of the distributions. These steps result in a sample of 101,672 observations covering the period 1966–2003.

Table 1, Panel A reports the distributional statistics of the overall sample. Market value per share (V) ranges from 0.094 to 143.125, with a mean (median) of 17.718 (12.250), all in dollars; book value per share (B) from 0.034 to 86.308, with a mean (median) of 10.918 (7.601); earnings per share (X) from −7.614 to 10.602, with a mean (median) of 1.055 (0.758); and profitability (q) (i.e., return on beginning book equity) from −3.083 to 1.166, with a mean (median) of 0.069 (0.112). The growth rate in equity book value (g) ranges from a low of −0.515 to a high of 4.004, with a mean (median) of 0.115 (0.041).

Table 1, Panel B provides the pairwise correlations among the variables. The market value of equity is significantly correlated with equity book value (with a correlation of 0.713), earnings (0.703), profitability (0.463), and growth (0.072), thus providing a preliminary indication of the valuation relevance of these variables. We also note that book value is highly correlated with

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[11] In Zhang (2000), equity book value is always understated under conservative accounting practices, and no cross-sectional relation is predicted between past investments and the valuation impact of equity book value.

[12] Our results are qualitatively unchanged if we alternatively use undiluted earnings per share, defined as earnings before extraordinary items (#18) divided by the number of common shares outstanding at the fiscal year-end (#25).

[13] Our results remain similar if we use year-end book values to define profitability.

[14] Compared with a measure of (output-based) earnings growth, our (input-based) growth proxy matches more closely with the construct in the original theoretical model in Zhang (2000). In robustness checks, we also use an *ex ante* growth measure and measures without inflation adjustment, and find the results similar.

TABLE 1
Descriptive Statistics

Panel A: Distributional Statistics

Variable	Mean	Median	Std.	Min.	Q1	Q3	Max.
V	17.718	12.250	17.691	0.094	5.000	24.750	143.125
B	10.918	7.601	10.894	0.034	3.509	14.507	86.308
X	1.055	0.758	1.717	−7.614	0.057	1.777	10.602
q	0.069	0.112	0.289	−3.083	0.019	0.190	1.166
g	0.115	0.041	0.366	−0.515	−0.036	0.154	4.004

Panel B: Correlations

Variable	V	B	X	q
B	0.713[a]			
X	0.703[a]	0.713[a]		
q	0.463[a]	0.238[a]	0.723[a]	
g	0.072[a]	−0.192[a]	0.022[a]	0.195[a]

[a] Indicates significance at the 0.01 level.

The sample consists of 101,672 firm-year observations from Compustat for the period 1966–2003. Market value per share (V) is the market price of common shares (Compustat data item #199). Book value per share (B) is calculated as the total book value of equity (#60) divided by the number of shares outstanding (#25) at the fiscal year-end. Earnings per share (X) is (diluted) earnings per share excluding extraordinary items (#57).

Profitability (q) is income before extraordinary items (#18) divided by the beginning book value of equity.

Growth opportunity (g) is the average annual growth rate of equity book value over the subsequent three years.

earnings, with a correlation of 0.713, versus 0.238 with profitability. The growth rate is significantly correlated with profitability, with a correlation of 0.195, confirming the economic intuition of "capital following profitability" (Biddle et al. 2001). Growth has a negative correlation with book value (−0.192).

IV. RESULTS BASED ON PARTIAL MODEL SPECIFICATIONS

To examine the valuation effects of investment growth (H1 to H4), we perform two sets of tests. One set of tests, provided in this section, uses "partial" specifications of the underlying valuation model, whereby we explicitly control for the variable needing to be held constant in a hypothesis. Partial models collapse one of the dimensions of the valuation space to view the valuation model from a particular angle. Thus, these simplified models highlight particular aspects of the growth effect. The limitation is that the individual hypotheses (concerning different aspects of the growth effect) are tested in separate regressions. The second set of tests, reported in subsequent sections, uses full model specifications, which allow multiple hypotheses to be tested jointly. This latter approach requires more complex specifications to integrate different parts of the growth effect into one regression.

When using partial model specifications, different methods are available to keep the control variable constant. Here, we choose to normalize (scale) the regression variables by the control variable, as in Burgstahler and Dichev (1997).[15] Because scaling by a negative number is not meaningful, for the analyses in this section we limit the sample to those observations with positive earnings (the sample size becomes 79,812).

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[15] In robustness checks, we also control for a variable by partitioning samples on the control variable into deciles and run separate regressions for the deciles, finding similar results.

To mitigate multicollinearity concerns, where applicable, we demean the continuous variables in the interaction terms in regressions in this section and in subsequent sections (Aiken and West 1991).

Results on the Relation between Equity Value and Earnings Given Book Value (H1)

H1 concerns the relation between V and X, given B. We first run the following regression each year from 1966 to 2003 to examine the basic V-X relation (which serves as a base model for introducing the growth variable later):

$$V_i/B_i = L_q * (\alpha_0 + \alpha_1 X_i/B_i) + M_q * (\beta_0 + \beta_1 X_i/B_i) + H_q * (\gamma_0 + \gamma_1 X_i/B_i) + e_i, \tag{2}$$

where V_i / B_i is the (per share) stock price of firm i at the end of a fiscal year scaled by the corresponding equity book value, and X_i / B_i is the earnings per share of firm i of a year scaled by the same equity book value. We use indicator variables L_q, M_q, and H_q to indicate observations in the high, medium, and low thirds of profitability in an annual sample, respectively. This specification follows Sirri and Tufano (1998), taking into account cross-correlations among the partitions. Following the predictions of an increasing and convex relation between V and X given B, we expect $0 \leq \alpha_1 \leq \beta_1 \leq \gamma_1$ with at least one of the inequalities strict.

Table 2 reports the average coefficients in Regression (2) across the 38 sample years, along

TABLE 2

Relation between Equity Value and Earnings, Given Equity Book Value

q-partitions	Median q	Intercept	X/B	Predicted Sign
L_q	0.061	1.302[a]	2.322[a]	+/0
		(9.58)	(4.79)	
M_q	0.142	0.477[a]	11.607[a]	+
		(3.12)	(19.66)	
H_q	0.255	0.086	15.424[a]	+
		(0.26)	(10.69)	
Diff. in X/B Coeff.: $M_q - L_q$			9.285[a]	+
			(10.48)	
Diff. in X/B Coeff.: $H_q - M_q$			3.817[a]	+
			(3.95)	
Diff. in X/B Coeff.: $H_q - L_q$			13.102[a]	+
			(9.23)	
Adj. R^2		0.72		

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[a] Indicates one-tailed significance at the 0.01 level.

This table reports the results of Regression (2):

$$V_i/B_i = L_q * (\alpha_0 + \alpha_1 X_i/B_i) + M_q * (\beta_0 + \beta_1 X_i/B_i) + H_q * (\gamma_0 + \gamma_1 X_i/B_i) + e_i.$$

We partition annual samples of positive earnings firms by profitability (q) into thirds and run regressions year by year, from 1966 to 2003. L_q (M_q, H_q) is an indicator variable for observations in the low- (medium-, high-) profitability region. The reported results are the average coefficients across the years, and t-values (in parentheses) are based on Fama-MacBeth standard errors with Newey-West adjustments.

with the Fama-MacBeth t-values with Newey-West adjustments. The slope coefficient on earnings is significantly positive in all profitability regions of our sample (restricted to positive-earnings observations in this section). The coefficient value is 2.322 in the low-profitability region, and increases to 11.607 and 15.4224 in the medium- and high-profitability regions, respectively. The difference in the slope coefficient between the high- and the low-profitability region (13.102) is significant ($t = 9.23$; $p < 0.01$). This initial analysis confirms the prior findings of an increasing and convex relation between *V* and *X*, controlling for *B* (e.g., Burgstahler and Dichev 1997; Collins et al. 1999).

To test H1, we extend Regression (2) to distinguish between low- and high-growth firms, as follows:

$$V_i/B_i = L_q * (\alpha_0 + \alpha_1 H_g + \alpha_2 X_i/B_i + \alpha_3 H_g X_i/B_i) + M_q * (\beta_0 + \beta_1 H_g + \beta_2 X_i/B_i + \beta_3 H_g X_i/B_i) + H_q * (\gamma_0 + \gamma_1 H_g + \gamma_2 X_i/B_i + \gamma_3 H_g X_i/B_i) + e_i, \quad (3)$$

where H_g is an indicator variable for observations in the high- (versus low-) growth group. Following H1, we expect $\gamma_3 > 0$, and $\gamma_3 \geq \beta_3 \geq \alpha_3$, with at least one of the inequalities strict.

Table 3 provides the results of Regression (3). In the high-profitability region, the incremental effect of high (versus low) growth on the earnings coefficient, captured by γ_3, has a positive value of 4.036 ($t = 5.44$), significant at the 0.01 level,[16] indicating that, in this region, growth significantly increases the slope of the value-earnings relation, controlling for equity book value. The effect of high (versus low) growth in the medium-profitability region decreases to 1.263 ($t = 2.59$), but remains significant at the 0.01 level. On the other hand, the effect of high growth in the low-profitability region becomes negative (-2.936), significant at the 0.01 level ($t = -8.32$). The difference in the slope effect of growth between the high- and medium-profitability regions is significant at the 0.05 level ($t = 2.41$), and that between the medium- and the low-profitability regions is significant at the 0.01 level ($t = 5.39$).

These results indicate that investment growth generally has a significant impact on the relation between equity value and earnings, given book value, but that the extent of the growth impact is contingent on the profitability level. Growth increases the value impact of earnings in high-profitability regions, but the effect becomes smaller and turns negative in lower-profitability regions, consistent with H1.

Results on the Relation between Equity Value and Book Value Given Earnings (H2 and H3)

H2 and H3 concern the relation between *V* and *B*, holding *X* constant. We first test H2, where the effect of investment growth is implied but not made explicit in the predicted equity value behavior. Following a design similar to that described above, we scale *V* and *B* by *X* and run the following regression each year from 1966 to 2003:

$$V_i/X_i = L_q * (\alpha_0 + \alpha_1 B_i/X_i) + M_q * (\beta_0 + \beta_1 B_i/X_i) + H_q * (\gamma_0 + \gamma_1 B_i/X_i) + e_i. \quad (4)$$

According to H2, the slope coefficient on book value will be positive in the low-*q* region, $\alpha_1 > 0$, and negative in the high-*q* region, $\gamma_1 < 0$. H2 also predicts $\alpha_1 \geq \beta_1 \geq \gamma_1$, with at least one of the inequalities strict.

Table 4 reports the results of Regression (4). As predicted, the coefficient on equity book value has a positive value of 0.668 ($t = 8.05$) in the low-profitability region, significant at the 0.01 level, whereas it has a negative value of -0.432 ($t = -2.39$) in the high-profitability region, significant at the 0.05 level. The coefficient in the medium region is 0.386 ($t = 2.39$), which is

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[16] Throughout the paper, we determine statistical significance with Bonferroni corrections, which require the hypothesized conditions to hold jointly in a regression.

TABLE 3

Growth and the Relation between Equity Value and Earnings, Given Equity Book Value (H1)

q-partitions	Median q	Median g $H_g=0$	Median g $H_g=1$	Intercept	H_g	X/B	H_gX/B	Predicted Sign	$X/B+H_gX/B$
L_q	0.061	−0.036	0.133	1.219^{a}	0.601^{a}	2.593^{a}	−2.936^{a},***	+/−	−0.343
				(10.54)	(6.60)	(5.98)	(−8.32)		(−0.36)
					[1.01]	[1.49]	[1.49]		
M_q	0.142	−0.014	0.116	1.624^{a}	0.450^{a}	9.557^{a}	1.263^{a},***	+	10.820^{a}
				(10.81)	(8.39)	(19.21)	(2.59)		(10.36)
					[1.01]	[2.01]	[2.00]		
H_q	0.255	−0.018	0.165	2.808^{a}	0.755^{a}	11.853^{a}	4.036^{a},***	+	15.889^{a}
				(9.83)	(12.56)	(13.82)	(5.44)		(13.12)
					[1.04]	[4.08]	[4.02]		
Diff. in X/B Coeff.: M_q-L_q							4.199^{a},***	+	
							(5.39)		
Diff. in X/B Coeff.: H_q-M_q							2.773^{a},**	+	
							(2.41)		
Diff. in X/B Coeff.: H_q-L_q							6.972^{a},***	+	
							(9.55)		
Adj. R^{2}					0.73				

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[a] Indicates one-tailed significance at the 0.01 level without Bonferroni corrections.

, * Indicate one-tailed significance at the 0.05 and 0.01 levels with Bonferroni corrections (m, the number of parts in the hypothesis, is 3), respectively.

This table reports the results of Regression (3):

$$V_i/B_i = L_q * (\alpha_0 + \alpha_1 H_g + \alpha_2 X_i/B_i + \alpha_3 H_g X_i/B_i) + M_q * (\beta_0 + \beta_1 H_g + \beta_2 X_i/B_i + \beta_3 H_g X_i/B_i) + H_q * (\gamma_0 + \gamma_1 H_g + \gamma_2 X_i/B_i + \gamma_3 H_g X_i/B_i) + e_i$$

where H_g is an indicator variable equal to 1 for firms in the high half of growth (measured by the average annual growth in equity book value in the subsequent three years) in a sample. The explanatory factors that are continuous variables are mean-adjusted. We partition annual samples of positive earnings firms by profitability (q) into thirds and run regressions year by year, from 1966 to 2003. L_q (M_q, H_q) is an indicator variable for observations in the low- (medium-, high-) profitability region. The reported results are the average coefficients across the years, and t-values (in parentheses) are based on Fama-MacBeth standard errors with Newey-West adjustments. The numbers in brackets are Variance Inflation Factors (VIF).

between those in the two extreme regions. The difference in the slope effect of growth between the high- and the medium-profitability regions is significant at the 0.01 level (t = −13.66), and that between the medium- and the low-profitability regions is also significant at the 0.01 level (t = −3.46). These results are consistent with the prediction in H2 that, given earnings, equity value is inversely related to equity book value among firms with high profitability, and positively related to book value among firms with low profitability.

To test H3, we extend Regression (4) to explicitly build in the growth effect in Regression (5) below:

TABLE 4

Relation between Equity Value and Equity Book Value, Given Earnings (H2)

q-partitions	Median q	Intercept	B/X	Predicted Sign
L_q	0.061	9.086^{a} (8.60)	0.668^{a,***} (8.05)	+
M_q	0.142	11.505^{a} (30.05)	0.386^{a,**} (2.39)	+/−
H_q	0.255	17.715^{a} (21.64)	−0.432^{a,**} (−2.39)	−
Diff. in B/X Coeff.: $M_q - L_q$			−0.282^{a,***} (−3.46)	−
Diff. in B/X Coeff.: $H_q - M_q$			−0.817^{a,***} (−13.66)	−
Diff. in B/X Coeff.: $H_q - L_q$			−1.100^{a,***} (−10.25)	−
Adj. R^2			0.78	

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[a] Indicates one-tailed significance at the 0.01 level without Bonferroni corrections.

, * Indicate one-tailed significance at the 0.05 and 0.01 levels with Bonferroni corrections (m = 4), respectively.

This table reports the results of Regression (4):

$$V_i/X_i = L_q * (\alpha_0 + \alpha_1 B_i/X_i) + M_q * (\beta_0 + \beta_1 B_i/X_i) + H_q * (\gamma_0 + \gamma_1 B_i/X_i) + e_i.$$

We partition annual samples of positive earnings firms by profitability (q) into thirds and run regressions year by year, from 1966 to 2003. L_q (M_q, H_q) is an indicator variable for observations in the low- (medium-, high-) profitability region. The reported results are the average coefficients across the years, and t-values (in parentheses) are based on Fama-MacBeth standard errors with Newey-West adjustments.

$$\begin{aligned} V_i/X_i = {} & L_q * (\alpha_0 + \alpha_1 H_g + \alpha_2 B_i/X_i + \alpha_3 H_g B_i/X_i) + M_q * (\beta_0 + \beta_1 H_g + \beta_2 B_i/X_i + \beta_3 H_g B_i/X_i) \\ & + H_q * (\gamma_0 + \gamma_1 H_g + \gamma_2 B_i/X_i + \gamma_3 H_g B_i/X_i) + e_i. \end{aligned} \tag{5}$$

According to H3, growth will increase the book value coefficient (i.e., make it more positive) when profitability is low ($\alpha_3 > 0$), but decrease it (i.e., make it more negative) when profitability is high ($\gamma_3 < 0$). H3 also predicts $\alpha_3 \geq \beta_3 \geq \gamma_3$ with at least one of the inequalities strict.

Table 5 reports the results of Regression (5). As predicted, in the low-profitability region, the effect of high growth on the book value coefficient is positive (0.163) and significant at the 0.01 level (t = 3.97), while its effect in the high-profitability region is negative (−0.374) and significant at the 0.01 level (t = −2.82). In the medium-profitability region, the effect of high growth is positive (0.114), but not statistically significant (t = 0.91). The difference of −0.537 in the slope effect between the high- and the low-profitability region is significant at the 0.01 level (t = −3.50). These results are consistent with H3, which predicts that growth will exhibit opposite effects on the *V-B* relation given *X* in the high- and low-profitability regions.

Results on the Relation between Equity Value and Book Value Given Profitability (H4)

According to H4, the *V-B* relation behaves very differently if we hold q, rather than X, constant. We adopt Equation (6) to examine the basic relation between V and B, controlling for q:

TABLE 5

Growth and the Relation between Equity Value and Equity Book Value, Given Earnings (H3)

q-partitions	Median q	Median g		Intercept	H_g	B/X	H_gB/X	Predicted Sign	$B/X + H_gB/X$
		$H_g = 0$	$H_g = 1$						
L_q	0.061	−0.036	0.133	20.165[a] (7.30)	4.893[a] (6.15) [1.02]	0.654[a] (7.88) [1.42]	0.163[a],*** (3.97) [1.43]	+	0.817[a] (11.93)
M_q	0.142	−0.014	0.116	13.592[a] (11.02)	2.580[a] (7.85) [1.01]	0.434[a] (4.29) [1.98]	0.114 (0.91) [1.97]	+/−	0.548[a] (3.73)
H_q	0.255	−0.018	0.165	13.281[a] (13.11)	2.767[a] (11.65) [1.02]	−0.035 (−0.47) [3.23]	−0.374[a],*** (−2.82) [3.20]	−	−0.409[a] (−3.77)
Diff. in H_g B/X Coeff.: $M_q - L_q$							−0.049 (−0.43)	−	
Diff. in H_g B/X Coeff.: $H_q - M_q$							−0.488[a],*** (−2.74)	−	
Diff. in H_g B/X Coeff.: $H_q - L_q$							−0.537[a],*** (−3.50)	−	
Adj. R^2							0.79		

[a] Indicates one-tailed significance at the 0.01 level without Bonferroni corrections.

*** Indicates one-tailed significance at the 0.01 level with Bonferroni corrections (m = 4).

This table reports the results of Regression (5):

$$V_i/X_i = L_q * (\alpha_0 + \alpha_1 H_g + \alpha_2 B_i/X_i + \alpha_3 H_g B_i/X_i) + M_q * (\beta_0 + \beta_1 H_g + \beta_2 B_i/X_i + \beta_3 H_g B_i/X_i) + H_q * (\gamma_0 + \gamma_1 H_g + \gamma_2 B_i/X_i + \gamma_3 H_g B_i/X_i) + e_i,$$

where H_g is an indicator variable equal to 1 for firms in the high half of growth (measured by the average annual growth in equity book value in the subsequent three years) in a sample. The explanatory factors that are continuous variables are mean-adjusted. We partition annual samples of positive earnings firms by profitability (q) into thirds and run regressions year by year, from 1966 to 2003. L_q (M_q, H_q) is an indicator variable for observations in the low- (medium-, high-) profitability region. The reported results are the average coefficients across the years, and t-values (in parentheses) are based on Fama-MacBeth standard errors with Newey-West adjustments. The numbers in brackets are Variance Inflation Factors (VIF).

$$V_i/q_i = L_q * (\alpha_0 + \alpha_1 H_b + \alpha_2 B_i/q_i + \alpha_3 H_b B_i/q_i) + M_q * (\beta_0 + \beta_1 H_b + \beta_2 B_i/q_i + \beta_3 H_b B_i/q_i)$$
$$+ H_q * (\gamma_0 + \gamma_1 H_b + \gamma_2 B_i/q_i + \gamma_3 H_b B_i/q_i) + e_i, \quad (6)$$

where H_b is an indicator variable for observations with equity book value above the sample median, which allows for possible changes in the slope across different B-regions.

Table 6 reports the results of Regression (6). The coefficient on equity book value is significantly positive in all three q-partitions. For low-q firms, the slope coefficient is 0.898 (t = 9.50) in the low-book-value region and 0.823 (t = 19.65) in the high-book-value region. These coefficients are 1.187 (t = 8.77) and 0.702 (t = 20.10) for medium-q firms, and 1.968 (t = 9.61) and 0.921 (t = 12.26) for high-q firms. These results confirm the basic intuition that, holding profitability constant, equity value will be higher for firms with more capital invested in operations. We also find that the slope becomes smaller as book value increases, mainly in higher profitability partitions. This may be a sign of diminishing returns to scale, indicating that the value derived from additional investment declines as scale expands.

To test H4, we modify Regression (6) to incorporate the growth effect as follows:

$$V_i/q_i = L_q * (\alpha_0 + \alpha_1 H_g + \alpha_2 B_i/q_i + \alpha_3 H_g B_i/q_i) + M_q * (\beta_0 + \beta_1 H_g + \beta_2 B_i/q_i + \beta_3 H_g B_i/q_i)$$
$$+ H_q * (\gamma_0 + \gamma_1 H_g + \gamma_2 B_i/q_i + \gamma_3 H_g B_i/q_i) + e_i. \quad (7)$$

As per H4, the incremental slope coefficient on book value is positive in all profitability ranges, $\alpha_3 > 0$, $\beta_3 > 0$, and $\gamma_3 > 0$.

Table 7 reports the results. The base coefficient on book value is significantly positive, equal to 0.884 (t = 9.20), 1.078 (t = 11.86), and 1.508 (t = 12.23) in the low-q, medium-q, and high-q regions, respectively. More importantly, the incremental slope effect of high growth is also significantly positive, equal to 0.050 (t = 5.19), 0.139 (t = 9.38), and 0.291 (t = 9.85), respectively, in these profitability regions, which further increases the coefficient on equity book value. These results are consistent with the prediction in H4 that growth will increase the (positive) slope in the relation between equity value and book value, given profitability.

A comparison between the results in Tables 6 and 7 here and those in Tables 4 and 5 above demonstrates that the behavior of the V-B relation is highly sensitive to whether we control for X or q in cross sections.

V. RESULTS BASED ON FULL MODEL SPECIFICATIONS

Thus far, we have tested H1 through H4 individually using partial models. We now test the hypotheses using "full" model specifications in which X and B may vary simultaneously. The use of a full model specification enables a more complete view of the behavior of equity value, allowing for testing the different aspects of the growth effect jointly within a common equation.

To design a regression specification for this purpose, we first rewrite theoretical model (1) as $V/B = P(q) + kq + gC(q)$, which is increasing and convex in q, and then proxy it empirically by a piecewise linear function of q that further distinguishes between high- and low-growth firms using H_g. Next, we multiply both sides of the simplified equation by B and rearrange the terms to recognize $qB = X$. These steps yield the following regression for explaining equity value:

$$V_i = \alpha_0 + \alpha_1 H_g + \alpha_2 H_q + \alpha_3 H_g H_q + \beta_0 B_i + \beta_1 H_g B_i + \beta_2 H_q B_i + \beta_3 H_g H_q B_i + \gamma_0 X_i + \gamma_1 H_g X_i$$
$$+ \gamma_2 H_q X_i + \gamma_3 H_g H_q X_i + e_i. \quad (8)$$

We run Regression (8) on annual samples, including firms with negative earnings as well as those with positive earnings, and calculate the average coefficients across the sample years together with the Fama-MacBeth t-values with Newey-West adjustments. As before, to mitigate multicollinear-

TABLE 6

Relation between Equity Value and Book Value, Given Profitability

		Median B								
q-partitions	Median q	$H_b = 0$	$H_b = 1$	Intercept	H_b	B/q	Predicted Sign	H_bB/q	$B/q + H_bB/q$	Predicted Sign
L_q	0.061	4.603	17.611	284.399[a]	−10.639[a]	0.898[a]	+	−0.075[a]	0.823[a],***	+
				(15.80)	(−3.93)	(9.50)		(−5.81)	(19.65)	
					[1.47]	[1.47]		[1.04]		
M_q	0.142	5.421	17.177	178.769[a]	1.591	1.187[a]	+	−0.485[a]	0.702[a],***	+
				(20.98)	(0.31)	(8.77)		(−4.70)	(20.10)	
					[9.30]	[9.68]		[7.40]		
H_q	0.255	4.871	14.477	117.600[a]	8.665[a]	1.968[a]	+	−1.047[a]	0.921[a],***	+
				(16.92)	(8.21)	(9.61)		(−4.19)	(12.26)	
					[2.85]	[8.83]		[5.70]		
Adj. R^2							0.87			

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[a] Indicates one-tailed significance at the 0.01 level.

*** Indicates one-tailed significance at the 0.01 level with Bonferroni corrections (m = 3).

This table reports the results of Regression (6):

$$V_i/q_i = L_q * (\alpha_0 + \alpha_1 H_b + \alpha_2 B_i/q_i + \alpha_3 H_b B_i/q_i) + M_q * (\beta_0 + \beta_1 H_b + \beta_2 B_i/q_i + \beta_3 H_b B_i/q_i) + H_q * (\gamma_0 + \gamma_1 H_b + \gamma_2 B_i/q_i + \gamma_3 H_b B_i/q_i) + e_i$$

where H_b is an indicator variable equal to 1 for firms in the high half of book value in a sample. The explanatory factors that are continuous variables are mean-adjusted. We partition annual samples of positive earnings firms by profitability (q) into thirds and run regressions year by year, from 1966 to 2003. L_q (M_q, H_q) is an indicator variable for observations in the low- (medium-, high-) profitability region. The reported results are the average coefficients across the years, and t-values (in parentheses) are based on Fama-MacBeth standard errors with Newey-West adjustments. The numbers in brackets are Variance Inflation Factors (VIF).

TABLE 7

Growth and the Relation between Equity Value and Equity Book Value, Given Profitability (H4)

		Median g							
q-partitions	**Median q**	**$H_g=0$**	**$H_g=1$**	**Intercept**	**H_g**	**B/q**	**H_gB/q**	**Predicted Sign**	**$B/q+H_gB/q$**
L_q	0.061	−0.036	0.133	267.631[a]	40.823[a]	0.884[a],***	0.050[a],***	+	0.934[a],***
				(16.76)	(7.74)	(9.20)	(5.19)		(20.69)
					[1.07]	[1.44]	[1.48]		
M_q	0.142	−0.014	0.116	158.428[a]	19.438[a]	1.078[a],***	0.139[a],***	+	1.217[a],***
				(19.94)	(9.07)	(11.86)	(9.38)		(21.66)
					[1.03]	[1.72]	[1.71]		
H_q	0.255	−0.018	0.165	93.935[a]	17.012[a]	1.508[a],***	0.291[a],***	+	1.799[a],***
				(18.13)	(4.15)	(12.23)	(9.85)		(22.51)
					[1.03]	[2.61]	[2.57]		
Adj. R^2						0.88			

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[a] Indicates one-tailed significance at the 0.01 level.

*** Indicates one-tailed significance at the 0.01 level with Bonferroni corrections (m = 6).

This table reports the results based on Regression (7):

$$V_i/q_i = L_q * (\alpha_0 + \alpha_1 H_g + \alpha_2 B_i/q_i + \alpha_3 H_g B_i/q_i) + M_q * (\beta_0 + \beta_1 H_g + \beta_2 B_i/q_i + \beta_3 H_g B_i/q_i) + H_q * (\gamma_0 + \gamma_1 H_g + \gamma_2 B_i/q_i + \gamma_3 H_g B_i/q_i) + e_i,$$

where the explanatory factors that are continuous variables are mean-adjusted. We partition annual samples of positive earnings firms by profitability (q) into thirds and run regressions year by year, from 1966 to 2003. L_q (M_q, H_q) is an indicator variable for observations in the low- (medium-, high-) profitability region. The reported results are the average coefficients across the years, and t-values (in parentheses) are based on Fama-MacBeth standard errors with Newey-West adjustments. The numbers in brackets are Variance Inflation Factors (VIF).

ity concerns, we use demeaned X and B in the regressions. The results of Regression (8) are reported in Table 8.

Testing H1, H2, and H3

Regression (8) allows for directly testing H1, H2, and H3, but not H4 (explained below). Our analysis here concerns whether the different parts of the growth effect stated in H1, H2, and H3 hold jointly, which amounts to a test of the "total" growth effect as represented by these hypotheses.[17] A joint test of these hypotheses requires stricter conditions for accepting them than in separate tests (through a Bonferroni adjustment), so accepting the hypotheses jointly automatically implies accepting them separately.

H1 predicts that growth will increase the slope of the *V-X* relation, given B, in the high-profitability region, $\gamma_1 + \gamma_3 > 0$, and that the growth effect on this slope will be greater in the high- than in the low-profitability region, $\gamma_3 > 0$. In Table 8, the estimate of $\gamma_1 + \gamma_3$ is 1.811 (t = 2.31), and that of γ_3 is 1.860 (t = 3.46). These coefficients have the predicted signs and are significant at the 0.1 level or better.

H2 predicts that (i) given X, V will decrease with B in the high-profitability region, which suggests either $\beta_0 + \beta_2 < 0$ (low growth), or $\beta_0 + \beta_1 + \beta_2 + \beta_3 < 0$ (high growth), or both; and (ii) given X, V will be an increasing function of B in the low-profitability region, which suggests either $\beta_0 > 0$ (low growth), or $\beta_0 + \beta_1 > 0$ (high growth), or both. The estimates are (i) $\beta_0 + \beta_2 = -0.074$ (t = −2.39), and $\beta_0 + \beta_1 + \beta_2 + \beta_3 = -0.257$ (t = −2.53); and (ii) $\beta_0 = 0.569$ (t = 4.68) and $\beta_0 + \beta_1 = 0.687$ (t = 4.70). These coefficients all have the predicted signs and are significant at the 0.05 level or better.

H3 predicts that growth (i) will reduce the slope of the *V-B* relation, given X, in the high-profitability region, $\beta_1 + \beta_3 < 0$, and (ii) will increase the slope in the low-profitability region, $\beta_1 > 0$. The results show that $\beta_1 + \beta_3 = -0.182$ (t = −2.32) and $\beta_1 = 0.117$ (t = 4.15). Again, the coefficients have the predicted signs and are significant at the 0.1 level or better.

Together, the results of Regression (8) are consistent with all aspects of the growth effect predicted in H1, H2, and H3 holding jointly (as well as separately).

In addition, our model (8) displays the valuation properties documented in prior studies. For example, the result that the slope coefficient on X is greater in the high- than in the low-profitability region, both for low- and high-growth firms, implies the convexity of the *V-X* relation, holding B constant (recognizing that $q=X/B$). Similarly, the result that the slope on B is greater in the low- than in the high-profitability region, for both low- and high-growth firms, implies the convexity of the *V-B* relation, holding X constant. The convexity of these relations has been shown by Burgstahler and Dichev (1997). Furthermore, these same results also mean that the valuation emphasis shifts from earnings to equity book value as we move from high-profitability to low-profitability firms, consistent with the finding of Collins et al. (1999) from their analysis of profit and loss firms.

Testing H4

H4 requires that q stay constant as B varies within a sample. This means that X ($X = Bq$) must vary with B proportionally when q stays fixed. Because we cannot test H4 from (8) for a general q-value,[18] we construct a test of H4 at two representative values of q, namely, the mean q-values

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[17] Again, tests here are based on Bonferroni-adjusted critical t-values. While H1, H2, and H3 together involve eight separate predictions (assuming that we test a strict version of H2, i.e., to test it separately for high and low growth), there are *six* independent restrictions, with the remaining two implied by the others. The tests are one-sided.

[18] The coefficient on B, given q, will be a function of the coefficients in Equation (8) and q. So without setting q to a specific value, neither this coefficient nor its t-value can be computed.

TABLE 8
Growth Effect in the Full Model (H1, H2, H3)

Variable	Coefficient	Predicted Sign	Estimate	t-value	VIF
Base Intercept			16.414[a]	10.50	
H_g			3.044[a]	14.99	2.158
H_q			3.046[a]	12.31	2.158
H_gH_q			1.034[a]	4.69	2.014
B [H2: slope in low g and low q]	β_0	+	0.569[a],***	4.68	4.761
H_gB [H3: growth effect in low q]	β_1	+	0.117[a],***	4.15	4.595
H_qB	β_2	−	−0.644[a]	−6.14	4.651
H_gH_qB	β_3	−	−0.299[a]	−5.31	4.751
X	γ_0	+	4.894[a]	9.89	5.093
H_gX [H1: growth effect in low q]	γ_1	+/−	−0.050	−0.19	5.067
H_qX	γ_2	+	6.776[a]	7.53	3.956
H_gH_qX [H1: incremental growth effect in high versus low q]	γ_3	+	1.860[a],***	3.46	5.084
$H_gX + H_gH_qX$ [H1: growth effect in high q]	$\gamma_1 + \gamma_3$	+	1.811[b],*	2.31	
$B + H_gB$ [H2: slope in high g and low q]	$\beta_0 + \beta_1$	+	0.687[a],***	4.70	
$B + H_qB$ [H2: slope in low g and high q]	$\beta_0 + \beta_2$	−	−0.074[a],**	−2.39	
$B + H_gB + H_qB + H_gH_qB$ [H2: slope in high g and high q]	$\beta_0 + \beta_1 + \beta_2 + \beta_3$	−	−0.257[a],**	−2.53	
$H_gB + H_gH_qB$ [H3: growth effect in high q]	$\beta_1 + \beta_3$	−	−0.182[b],*	−2.32	
Adj. R^2			0.63		

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[a], [b] Indicate one-tailed significance at the 0.01 and 0.05 levels without Bonferroni corrections, respectively.
*, **, *** Indicate one-tailed significance at the 0.1, 0.05, and 0.01 levels with Bonferroni corrections (m = 6), respectively.

(continued on next page)

TABLE 8 (continued)

This table reports the results of Regression (8):

$$V_i = \alpha_0 + \alpha_1 H_g + \alpha_2 H_q + \alpha_3 H_g H_q + \beta_0 B_i + \beta_1 H_g B_i + \beta_2 H_q B_i + \beta_3 H_g H_q B_i + \gamma_0 X_i + \gamma_1 H_g X_i + \gamma_2 H_q X_i + \gamma_3 H_g H_q X_i + e_i,$$

where H_q is an indicator variable equal to 1 for firms in the high half of profitability in a sample, and H_g is an indicator variable equal to 1 for firms in the high half of growth. The explanatory factors that are continuous variables are mean-adjusted. We run regressions year by year, from 1966 to 2003. The reported results are the average coefficients across the years, and t-values are based on Fama-MacBeth standard errors with Newey-West adjustments.

in the low- and high-q regions. Table 9 presents the results.

At the mean q-value in the low-profitability region ($q = -0.090$), the coefficient on B, as implied by Regression (8), equals 0.980 (t = 14.88); this is the slope of the V-B relation for low-growth firms. At the same q-value, the slope increases to 1.298 (t = 12.47) for high-growth firms; the coefficient difference of 0.318 (t = 7.15) is significant at the 0.01 level.

The results are qualitatively the same in the high-profitability region. At the mean q-value in this region ($q = 0.229$), the coefficient on B is 2.089 (t = 21.42) for low-growth firms, and increases to 2.217 (t = 22.82) for high-growth firms; the difference of 0.128 (t = 3.07) is significant at the 0.01 level. These results are consistent with H4, which predicts that the slope of the V-B relation will be steeper for high-growth than for low-growth firms.

Economic Importance of the Growth Effect

To assess the economic importance of the growth effect, we address two specific issues in this subsection: (i) how much of a difference the growth variable makes in explaining variations in equity value induced by earnings or book value, and (ii) how important it is to condition the growth effect on profitability. As in our hypothesis testing, we focus on the growth effect conveyed through the slope coefficients on earnings and book value.

Economic Effect of Growth on Predicted Value Changes Induced by Earnings

We first illustrate the importance of the growth variable in predicting equity value changes induced by earnings, at a given book value (set at the overall sample mean, \$10.918). In the high-q region, the X-value corresponding to the 3rd quartile of q in the sample ($q = 0.190$) is \$2.074. We pick two earnings points at \$0.5 below and above this amount, that is, X = \$1.574 and \$2.574, respectively. Following the results of Regression (8), as X varies from \$1.574 to \$2.574 holding B

TABLE 9

Growth and the Relation between Equity Value and Book Value at Low and High Profitability Points (H4)

q-value		Predicted Sign	B Coefficient	t-value
$q_L = -0.090$	Slope in low g region	+	0.980^{a},***	14.88
	Incremental slope in high g	+	0.318^{a},***	7.15
	Slope in high g	+	1.298^{a},***	12.47
$q_H = 0.229$	Slope in low g region	+	2.089^{a},***	21.42
	Incremental slope in high g	+	0.128^{a},***	3.07
	Slope in high g	+	2.217^{a},***	22.82

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[a] Indicates one-tailed significance at the 0.01 level without Bonferroni corrections.

*** Indicates one-tailed significance at the 0.01 level with Bonferroni corrections (m = 4).

This table reports results for H4 (at specific q-values) that are derived from Regression (8) (Table 8):

$$V_i = \alpha_0 + \alpha_1 H_g + \alpha_2 H_q + \alpha_3 H_g H_q + \beta_0 B_i + \beta_1 H_g B_i + \beta_2 H_q B_i + \beta_3 H_g H_q B_i + \gamma_0 X_i + \gamma_1 H_g X_i + \gamma_2 H_q X_i + \gamma_3 H_g H_q X_i + e_i.$$

We select the mean q-values in the low- and high-q groups, denoted q_L and q_H, respectively, and determine the slope of the V-B relation at these points. For the low-profitability group, the slope of B is $\beta_0 + \gamma_0 q_L$ for low-growth firms, and $\beta_0 + \beta_1 + (\gamma_0 + \gamma_1) q_L$ for high-growth firms. For the high-profitability group, the slope of B is $\beta_0 + \beta_2 + (\gamma_0 + \gamma_2)\ q_H$ for low-growth firms, and $\beta_0 + \beta_1 + \beta_2 + \beta_3 + (\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3) q_H$ for high-growth firms. The results are averaged across the years, and t-values are based on Fama-MacBeth standard errors with Newey-West adjustments.

at \$10.918, the predicted V-value moves from \$25.517 to \$37.187 for low-growth firms ($H_g = 0$), an increase of \$11.67 (=37.187 − 25.517).[19] For the same variation in X, the predicted V moves from \$30.534 to \$44.014 for high-growth firms ($H_g = 1$), an increase of \$13.48. The difference in the predicted V-change between low- and high-growth firms is \$1.81, which is about 5 percent of the value of the underlying firms, an economically significant amount. For reference, the profitability levels associated with the two earnings points above are 0.144 and 0.236, respectively, a difference of 0.092, which is relatively small as compared to the standard deviation of 0.289 in the overall sample.

In the low-q region, we choose X-values at \$0.5 below and above the 1st q-quartile ($q = 0.019$), that is, $X = -\$0.293$ and \$0.707, respectively. Following the results of Regression (8), as X varies from–\$0.293 to \$0.707 holding B at \$10.918, the predicted V moves from \$9.817 to \$14.711 in the low-growth group, an increase of \$4.894. For the same variation in earnings, the predicted equity value moves from \$12.928 to \$17.772 in the high-growth group, an increase of \$4.844, about the same as in the low-growth group. The difference between these two predicted value changes of–\$0.05 is less than 0.4 percent of the value of the underlying firms. Thus, in low-profitability regions, growth makes almost no difference economically in predicting equity value changes induced by earnings. The contrasting results between high- and low-profitability firms highlight the role of profitability in determining the growth effect in valuation, as H1 predicts.

Economic Effect of Growth on Predicted Value Changes Induced by Equity Book Value

We similarly illustrate the growth effect on predicted value changes induced by book value, given earnings. In the high-q region, we set X at \$2.0 and choose B at \$3 below and \$3 above the sample mean of \$10.918; that is, $B=$ \$7.918 and \$13.918, respectively. The corresponding q-values are 0.253 and 0.144. Among low-growth firms, as B moves from \$7.918 to \$13.918 holding X at \$2.0, the predicted V decreases from \$30.713 to \$30.263, a small reduction of \$0.45. Among high-growth firms, the same change in B causes the predicted V to decrease from \$37.048 to \$35.505, a more significant reduction of \$1.543. The *difference* in the predicted V-change between the high- and low-growth firms is \$1.093, about 3 percent of the underlying equity value, which is economically important.

In the low-q region, we set X at –\$0.5 (loss firms) and again choose B at \$3 below and above the sample mean; that is, $B =$ \$7.918 and \$13.918, respectively. The corresponding q-values are −0.063 and −0.036. Among low-growth firms, as B moves from \$7.918 to \$13.918 holding X at –\$0.5, the predicted V changes from \$7.097 to \$10.511, an increase of \$3.414. On the other hand, among high-growth firms, the same variation in B causes the predicted V to change from \$9.868 to \$13.984, an increase of \$4.116. In this case, the *difference* in the predicted V-change between high- and low-growth firms is \$0.702, about 7 percent of the underlying firms' equity value, an economically significant amount.

To summarize, in the high-q region increases in equity book value cause faster equity value declines among high- (versus low-) growth firms, whereas in the low-q region, increases in equity book value cause equity value to rise faster among high- (versus low-) growth firms.

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[19] In this subsection, all equity values are calculated based on the estimated Equation (8) (reported in Table 8): $V = 16.414 + 3.044H_g + 3.046H_q + 1.034H_gH_q + 0.569B' + 0.117H_gB' - 0.644H_qB' - 0.299H_gH_qB' + 4.894X' - 0.05H_gX' + 6.776H_qX' + 1.86H_gH_qX'$, where $X' = X - 1.055$ and $B' = B - 10.918$ are mean-adjusted earnings and equity book value, respectively. For example, the predicted V at $X = 1.574$ conditional on $B = 10.918$, $H_g = 0$, and $H_q = 1$ is $V = 16.414 + 3.046 + (0.569 - 0.644) \times (10.918 - 10.918) + (4.894 + 6.776) \times (1.574 - 1.055) = 25.517$; and that at $X = 2.574$ is $V = 16.414 + 3.046 \div (0.569 - 0.644) \times (10.918 - 10.918) + (4.894 + 6.776) \times (2.574 - 1.055) = 37.187$.

VI. CONSERVATISM, PAST INVESTMENT ACTIVITIES, AND THE RELATION BETWEEN EQUITY VALUE AND ACCOUNTING DATA

To test H5 concerning the role of past investments that arises from accounting conservatism, we split annual samples into halves based on the rate of past investment increases, measured by the average annual change in capital expenditures scaled by the year-beginning equity book value over the past three years (the further data requirement reduces the sample to 76,662 observations).[20] Following H5, firms with faster (slower) investment increases have high (low) earnings conservatism, labeled *Hc* (*Lc*). The following regression, generalized from (8), estimates a separate value-accounting relation for the *Lc* and *Hc* groups:

$$\begin{aligned}V_i = L_c\{&\alpha_{0L} + \alpha_{1L}H_g + \alpha_{2L}H_q + \alpha_{3L}H_gH_q + \beta_{0L}B_i + \beta_{1L}H_gB_i + \beta_{2L}H_qB_i + \beta_{3L}H_gH_qB_i + \gamma_{0L}X_i \\ &+ \gamma_{1L}H_gX_i + \gamma_{2L}H_qX_i + \gamma_{3L}H_gH_qX_i\} + H_c\{\alpha_{0H} + \alpha_{1H}H_g + \alpha_{2H}H_q + \alpha_{3H}H_gH_q + \beta_{0H}B_i \\ &+ \beta_{1H}H_gB_i + \beta_{2H}H_qB_i + \beta_{3H}H_gH_qB_i + \gamma_{0H}X_i + \gamma_{1H}H_gX_i + \gamma_{2H}H_qX_i + \gamma_{3H}H_gH_qX_i\} + e_i. \quad (9)\end{aligned}$$

In (9), the three-way sorting by conservatism, growth, and profitability is performed independently of one another. The purpose of (9) is twofold: (i) to examine the validity of the predicted growth effect (H1, H2, and H3) in a more refined setting that differentiates the high-earnings-conservatism group from the low group, and (ii) to examine the role of earnings conservatism (H5) in a regression that incorporates the growth effect. Table 10 reports the results of Regression (9), as explained below.

Growth Effect in the Low-Conservatism Group

Table 10, Panel A reports the results on the growth effect in the low-conservatism group. In the high-profitability region, the effect of high (versus low) investment growth on the slope of the *V-X* relation given *B*, $\gamma_{1L} + \gamma_{3L}$, has a value of 2.917 (t = 3.52), significant at the 0.01 level.[21] This effect decreases to 0.386 (t = 0.82) in the low-profitability region. The difference between them (γ_{3L}) of 2.531 (t = 6.82) is significant at the 0.01 level, consistent with H1.

In the high-profitability region, the slope of the *V-B* relation given *X* is represented by $\beta_{0L} + \beta_{2L}$ for low-growth firms, which has a negative value of −0.187 (t = −5.50), whereas the slope is represented by $\beta_{0L} + \beta_{1L} + \beta_{2L} + \beta_{3L}$ for high-growth firms, which has an even more negative value of −0.506 (t = −7.21). The difference between them ($\beta_{1L} + \beta_{3L}$) of −0.319 (t = −3.39) is significant at the 0.01 level.

In contrast, in the low-profitability region, the slope of the *V-B* relation for low-growth firms (β_{0L}) has a positive value of 0.528 (t = 4.05), and that for high-growth firms ($\beta_{0L} + \beta_{1L}$) is 0.538 (t = 3.17), consistent with H2. The slope difference between high- and low-growth firms (β_{1L}) of 0.010 (t = 0.25) is statistically insignificant. These results are consistent with H2 and H3(i), but inconsistent with H3(ii).

Overall, the above results are consistent with all parts of the growth effect, stated in H1, H2, and H3, that are predicted by Zhang's (2000) model. However, we find no evidence for the intuitive prediction in part (ii) of H3.

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[20] In previous sections where we examine the effect of investment growth, what is a proper measure of investment is less clear-cut. There, we use changes in equity book value to proxy for investment (with alternative proxies used in robustness checks). This can be justified on the ground that financial (as well as operating) assets are part of investment because they generate earnings themselves and can be used to fund future operations. But in this section, "investment" is more clearly defined in Zhang (2000), which refers to assets depreciated over multiple periods. It follows that, here, capital expenditures better proxy for investment than alternatives such as equity book value changes.

[21] Again, we apply Bonferroni corrections to critical t-values here, recognizing all the conditions imposed by H1, H2, and H3, and requiring them to hold in both the low- and high-conservatism groups.

TABLE 10

Conservatism and Growth Effects in the Full Model (H1, H2, H3, H5)

Panel A: Low Earnings Conservatism (L_c) Group

Variable	Coefficient	Predicted Sign	Estimate	t-value	VIF
Base Intercept			15.113^{a}	8.66	
H_g			1.679^{a}	9.02	2.503
H_q			−0.312	−0.82	2.910
H_gH_q			−0.990^{a}	−3.21	2.721
B [H2: slope in low g and low q]	β_{0L}	+	0.528^{a},***	4.05	4.562
H_gB [H3: growth effect in low q]	β_{1L}	+	0.010	0.25	4.300
H_qB	β_{2L}	−	−0.715^{a}	−4.53	5.120
H_gH_qB	β_{3L}	−	−0.329^{a}	−5.92	5.127
X	γ_{0L}	+	5.993^{a}	13.28	5.520
H_gX [H1: growth effect in low q]	γ_{1L}	+/−	0.386	0.82	5.496
H_qX	γ_{2L}	+	7.542^{a}	4.84	5.030
H_gH_qX [H1: incremental growth effect in high versus low q]	γ_{3L}	+	2.531^{a},***	6.82	6.620
$H_gX + H_gH_qX$ [H1: growth effect in high q]	$\gamma_{1L} + \gamma_{3L}$	+	2.917^{a},***	3.52	
$B + H_gB$ [H2: slope in high g and low q]	$\beta_{0L} + \beta_{1L}$	+	0.538^{a},***	3.17	
$B + H_qB$ [H2: slope in low g and high q]	$\beta_{0L} + \beta_{2L}$	−	−0.187^{a},***	−5.50	
$B + H_gB + H_qB + H_gH_qB$ [H2: slope in high g and high q]	$\beta_{0L} + \beta_{1L} + \beta_{2L} + \beta_{3L}$	−	−0.506^{a},***	−7.21	
$H_gB + H_gH_qB$ [H3: growth effect in high q]	$\beta_{1L} + \beta_{3L}$	−	−0.319^{a},***	−3.39	

Panel B: High Earnings Conservatism (H_c) Group

Variable	Coefficient	Predicted Sign	Estimate	t-value	VIF
Base Intercept			19.159^{a}	10.06	
H_g			2.861^{a}	17.05	2.218

(continued on next page)

Panel B: High Earnings Conservatism (H_c) Group

Variable	Coefficient	Predicted Sign	Estimate	t-value	VIF
H_q			2.943^{a}	12.64	2.422
H_gH_q			0.906^{a}	3.02	2.223
B [H2: slope in low g and low q]	β_{0H}	+	0.426^{a},**	3.04	4.764
H_gB [H3: growth effect in low q]	β_{1H}	+	−0.044	−0.87	4.799
H_qB	β_{2H}	−	-0.799^{a}	−5.36	4.561
H_gH_qB	β_{3H}	−	-0.415^{a}	−9.27	4.749
X	γ_{0H}	+	6.895^{a}	11.76	5.902
H_gX [H1: growth effect in low q]	γ_{1H}	+/−	0.466	0.99	5.942
H_qX	γ_{2H}	+	7.827^{a}	5.31	4.538
H_gH_qX [H1: incremental growth effect in high versus low q]	γ_{3H}	+	2.552^{a},***	8.00	5.836
$H_gX + H_gH_qX$ [H1: growth effect in high q]	$\gamma_{1H} + \gamma_{3H}$	+	3.018^{a},***	5.45	
$B + H_gB$ [H2: slope in high g and low q]	$\beta_{0H} + \beta_{1H}$	+	0.382^{a},***	4.71	
$B + H_qB$ [H2: slope in low g and high q]	$\beta_{0H} + \beta_{2H}$	−	-0.373^{a},***	−13.10	
$B + H_gB + H_qB + H_gH_qB$ [H2: slope in high g and high q]	$\beta_{0H} + \beta_{1H} + \beta_{2H} + \beta_{3H}$	−	-0.833^{a},***	−10.44	
$H_gB + H_gH_qB$ [H3: growth effect in high q]	$\beta_{1H} + \beta_{3H}$	−	-0.459^{a},***	−3.93	
Adj. R^2			0.87		

Panel C: Testing H5 (comparing earnings coefficients between H_c and L_c)

Variable	Predicted Sign	Estimate	t-value
Diff. in X coeff. in (low g, low q): $H_c - L_c$	+	0.902^{a},***	3.46
Diff. in X coeff. in (high g, low q): $H_c - L_c$	+	0.982^{a},***	3.16
Diff. in X coeff. in (low g, high q): $H_c - L_c$	+	1.188^{a},**	2.56
Diff. in X coeff. in (high g, high q): $H_c - L_c$	+	1.289^{a},***	3.31

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[a] Indicates one-tailed significance at the 0.01 level without Bonferroni corrections.

, * Indicate one-tailed significance at the 0.05 and 0.01 levels with Bonferroni corrections (m = 12 for testing the growth effect and m = 4 for testing the conservatism effect), respectively.

(continued on next page)

This table reports the results of Regression (9):

$$V_i = L_c\{\alpha_{0L} + \alpha_{1L}H_g + \alpha_{2L}H_q + \alpha_{3L}H_gH_q + \beta_{0L}B_i + \beta_{1L}H_gB_i + \beta_{2L}H_qB_i + \beta_{3L}H_gH_qB_i + \gamma_{0L}X_i + \gamma_{1L}H_gX_i + \gamma_{2L}H_qX_i + \gamma_{3L}H_gH_qX_i\} + H_c\{\alpha_{0H} + \alpha_{1H}H_g + \alpha_{2H}H_q + \alpha_{3H}H_gH_q + \beta_{0H}B_i$$
$$+ \beta_{1H}H_gB_i + \beta_{2H}H_qB_i + \beta_{3H}H_gH_qB_i + \gamma_{0H}X_i + \gamma_{1H}H_gX_i + \gamma_{2H}H_qX_i + \gamma_{3H}H_gH_qX_i\} + e_i,$$

where $L_c(H_c)$ indicates firms with low (high) earnings conservatism, which are firms having average investment increases over the past three years below (above) the sample median, with investment increases measured by changes in capital expenditures scaled by equity book value. H_q is an indicator variable equal to 1 for firms in the high half of profitability in a sample, with profitability defined as earnings per share divided by equity book value per share at the beginning of the year. H_g is an indicator variable equal to 1 for firms in the high half of growth, with growth measured as the average annual growth rate of equity book value in the subsequent three years. We run regressions year by year, from 1968 to 2003. The reported results are the average coefficients across the years, and t-values are based on Fama-MacBeth standard errors with Newey-West adjustments.

Growth Effect in the High-Conservatism Group

The same qualitative properties hold in the high-earnings-conservatism group, as shown in Table 10, Panel B. In the high-profitability region, the effect of high (versus low) growth on the slope of the V-X relation ($\gamma_{1H} + \gamma_{3H}$) is 3.018 (t = 5.45); this effect decreases to 0.466 (t = 0.99) in the low-profitability region. The difference (γ_{3H}) is 2.552 (t = 8.00), significant at the 0.01 level.

In the high-profitability region, the V-B relation given X has a negative slope ($\beta_{0H} + \beta_{2H}$) equal to −0.373 (t = −13.10) for low-growth firms, which turns even more negative for high-growth firms ($\beta_{0H} + \beta_{1H} + \beta_{2H} + \beta_{3H}$), equal to −0.833 (t = −10.44). The difference in slope between them ($\beta_{1H} + \beta_{3H}$) is −0.459 (t = − 3.93), significant at the 0.01 level. On the other hand, in the low-profitability region, the V-B relation given X has a positive slope both for low-growth firms (β_{0H}), equal to 0.426 (t = 3.04), and for high-growth firms ($\beta_{0H} + \beta_{1H}$), equal to 0.382 (t = 4.71). The slope difference between them (β_{1H}) of −0.044 (t = −0.87) is insignificant.

Thus, as in the low-conservatism group, the results are consistent with all parts of the growth effect stated in H1, H2, and H3 that are based on theoretical predictions, but no evidence is found for the intuitive prediction in part (ii) of H3.

Conservatism and the Valuation Impact of Earnings (H5)

H5 predicts that firms that have just experienced rapid increases in investments will have their earnings more conservatively stated than do those that experienced slow (or negative) increases in recent investments. Thus, *ceteris paribus*, the earnings coefficient should be greater in the former than in the latter group. Regression (9) contains four different combinations of g and q (as measured by their respective indicator variables); a separate earnings coefficient is estimated for each combination. Table 10, Panel C shows that the earnings coefficient is greater in the high- than in the low-conservatism group in all four scenarios identified by (g, q) combinations. The coefficient difference between the high- and low-conservatism groups is 0.902 (t = 3.46) in scenario (*Low g, Low q*); 0.982 (t = 3.16) in (*High g, Low q*); 1.188 (t = 2.56) in (*Low g, High q*); and 1.289 (t = 3.31) in (*High g, High q*). These differences are significant at the 0.05 level or better, consistent with H5.[22]

VII. ROBUSTNESS CHECKS

We also perform a range of robustness checks employing alternative variable measures and regression specifications, and find the qualitative results generally unchanged.

In our main analysis, growth is measured by average *ex post* growth in equity book value. Alternative measures we use are *ex post* growth in total assets, *ex post* growth in operating assets, and *ex post* capital expenditures (scaled by property, plant, and equipment). In addition to using a simple high-low indicator variable to measure growth, we also use the percentile rankings of growth in our tests and obtain similar results.

Beyond these *ex post* growth proxies, we also use the median analyst forecast of long-term earnings growth in an industry to proxy for the expected investment growth of firms in that industry. This *ex ante* measure mitigates the concern of survivorship biases potentially introduced by *ex post* proxies. While we have adjusted growth for inflation (the Consumer Price Index) in the main analysis, we also conduct an analysis without this adjustment.

In our tests using partial model specifications (Section IV), we control for a variable by the method of scaling. Alternatively, we also use the method of partitioning, whereby we partition annual samples along the dimension of the control variable (to reduce the extent of its variation) and then run regressions separately for the partitions. With this alternative method, we are able to include negative- as well as positive-earnings observations in the tests. Our results are also consistent with the predicted growth effect.

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[22] For each given (g, q)-combination, we find generally little difference in mean (median) q- and g-values between the L_c and H_c groups.

The theoretical model underlying our study assumes the clean surplus relation. As Ohlson (2005) points out, in situations in which a firm engages in equity transactions with investors that change the number of shares outstanding, the clean surplus relation does not hold on a per-share basis, and should be applied to the whole firm (using total equity value, book value, and earnings). To mitigate this concern, we run two additional sets of regressions. In one set, we use total equity value, equity book value, and earnings instead of per-share amounts. In the second set, we restrict the sample to those firms that experience a change of less than 1 percent in the number of shares outstanding between the preceding and the subsequent year (and so the clean surplus relation approximately holds on a per-share basis). In both sets of analysis, the results are similar to those reported above.

In Section VI, we proxy past investment by capital expenditures scaled by equity book value. We alternatively use PPE and total assets as scalars and find similar results.

VIII. SUMMARY AND CONCLUSIONS

This study incorporates the role of investment growth to reexamine the relation between equity value, earnings, and equity book value. Following the valuation model of Zhang (2000), we predict how investment growth, in conjunction with profitability, influences this relation. We then empirically test the predictions and find consistent results. In addition to exploring the valuation effect of prospective investments, which arises from economic incentives for value creation, this study also demonstrates how past investments in conjunction with conservative accounting practices affect the valuation role of earnings. Our results provide insight into the important role of investment activities in mediating the value-accounting relation.

Valuation research is of interest to investors, standard-setters, and academic researchers. In practice, investors and analysts adopt different valuation techniques and emphasize various fundamental factors in financial forecasting. By presenting a more complete view of the relation between equity value and accounting data, our study provides a framework for thinking about how to perform financial analysis and valuation. In particular, it explains that both prospective investment growth and past investment activities alter the way accounting data are mapped into value. Relative valuation techniques, such as price-to-earnings and price-to-book multiples, are widely used in investment and business decisions. By demonstrating how the relation between equity value and earnings, and similarly that between equity value and book value, depends on investment growth, the study gives a better understanding of the fundamental determinants of these multiples. This is useful not only for identifying benchmark firms in multiples-based valuations, but also for determining the appropriate levels for the multiples. Likewise, for standard-setters who are responsible for designing reporting rules to enhance the usefulness of financial data, a more in-depth knowledge of the link between accounting data and value will be beneficial for formulating more targeted standards.

In accounting research, parsimonious valuation models are often used in which earnings and equity book value serve as explanatory variables for equity value. However, the circumstances in which such simplified models are appropriate are not always well understood. Our study shows that the relation between equity value and accounting data is more complex than what has been previously documented or assumed in the literature; in particular, growth is an important attribute that intervenes in this relation. The study also shows that in cross-sections, the behavior of equity value in relation to a specific accounting variable (such as equity book value) can differ substantially along different dimensions, such that it is plausible to obtain different, or even opposite, results from different empirical samples.

Finally, our results have implications for research that applies valuation models to address accounting-related issues. Given our finding that the valuation impacts of earnings and book value depend on investment activities, one needs to be cautious when drawing inferences concerning, for example, the value relevance of accounting data or the degree of accounting conservatism. In such studies, researchers should separate the effect of a firm's economic condition from that of accounting.

APPENDIX
MATHEMATICAL DERIVATIONS OF THE GROWTH EFFECT

This appendix presents the mathematical derivations to show how investment growth affects the relation between equity value and accounting variables (H1, H3, and H4). Note that H2 was derived in Zhang (2000, 284–285).

Growth and the Relation between Equity Value and Earnings, Given Book Value (H1)

Differentiating V given by model (1) with respect to X, holding B constant, we get $\frac{dV}{dX}\big|_B = P'(q) + k + gC'(q) > 0$. Differentiating $\frac{dV}{dX}\big|_B$ with respect to g yields $d\left(\frac{dV}{dX}\big|_B\right) / dg = C'(q) > 0$. H1 follows by further noting that $C'(q)$ increases with q.

Growth and the Relation between Equity Value and Book Value, Given Earnings (H3)

Differentiating V with respect to B, given X, and simplifying, we have $\frac{dV}{dB}\big|_X = -qP'(q) + P(q) - gqC'(q) + gC(q)$. Differentiating $\frac{dV}{dB}\big|_X$ with respect to g yields $d\left(\frac{dV}{dB}\big|_X\right) / dg = -qC'(q) + C(q)$, which is negative when q is sufficiently large to make the growth option valuable. This leads to part (i) of H3, relevant to high-profitability regions. The model does not address the case of high growth by low-profitability firms, as related to part (ii) of H3.

Growth and the Relation between Equity Value and Book Value, Given Profitability (H4)

Differentiating V with respect to B, given q, we get $\frac{dV}{dB}\big|_q = P(q) + kq + gC(q) > 0$, indicating that the marginal valuation impact of book value, given profitability, is a function of q and g. Differentiating $\frac{dV}{dB}\big|_q$ with respect to g, we get $d\left(\frac{dV}{dB}\big|_q\right) / dg = C(q) > 0$. Part (i) of H4 follows. Again, the model does not address the case of high growth by low-profitability firms, as related to part (ii) of H4.

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Voluntary Disclosure to Influence Investor Reactions to Merger Announcements: An Examination of Conference Calls

Michael D. Kimbrough
University of Maryland
Henock Louis
The Pennsylvania State University

ABSTRACT: We find that bidders are more likely to hold conference calls at merger announcements when the mergers are financed with stock and when the transactions are large. After controlling for endogeneity, we also find that conference calls are associated with more favorable market reactions to merger announcements. A content analysis of merger-related information releases for a limited subsample indicates that the more favorable reaction is related to the fact that, compared to press releases, conference calls provide a greater volume of information and place greater emphasis on forward-looking details. We find no evidence that the superior announcement returns associated with conference calls subsequently reverse or that conference calls are positively associated with pre-merger announcement abnormal accruals. Overall, the results suggest that managers use conference calls around merger announcements to credibly convey favorable private information to the market.

Keywords: *conference calls; voluntary disclosure; mergers.*

JEL Classifications: *D82; G34; M41.*

I. INTRODUCTION

We examine the determinants and consequences of acquirers' decisions to supplement merger announcement press releases with conference calls. Our analysis of disclosure choices concurrent with merger announcements is based on the potentially important role of disclosure in influencing investors' responses to merger announcements. For example, Wayne Moore, former president of Goldman Sachs, argues:

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> It is critical that the announcement of a transaction be well received and that the bidder's stock reacts favorably. As a result, the time, effort, and care that goes into announcing a deal has increased significantly. And the content—the description of the strategic rationale and the quantification of the synergies and future earnings effects—has as well. (Moore et al. 1998, 12)

Lipin and Sirower (2003) maintain that effectively communicating a merger plan to investors involves disclosing specific and verifiable details about the integration plans, financial projections (including whether the merger is accretive or dilutive to earnings), the management team type, key assumptions underlying the anticipated success, and the basis for the purchase price. The interactive nature of conference calls provides managers with an ideal forum for communicating the kind of contextual information that Lipin and Sirower (2003) argue is necessary to gain investor confidence. A detailed content analysis of a limited subsample of merger announcement press releases and conference call transcripts confirms that bidders that hold conference calls at the time of their merger announcements provide a greater volume of disclosure and emphasize forward-looking details to a greater degree than bidders that rely solely on merger announcement press releases to introduce the proposed deals to the market.

While we expect managers in general to have incentives to effectively communicate the rationale for their proposed transactions due to the well-documented cost of capital benefits of forthcoming disclosure, we examine the circumstances in which these incentives are particularly pronounced in the merger and acquisition (M&A) context. Specifically, we expect disclosure incentives to be more pronounced for stock-based mergers because the costs of a stock-based merger and the likelihood of its completion are directly tied to the post-announcement value of the bidder's stock. Moreover, there is a general presumption that stock-for-stock mergers are often motivated by overvaluation as opposed to genuine business considerations (Shleifer and Vishny 2003). Therefore, managers who have valid economic rationales to undertake a stock-for-stock merger have incentives to provide additional information to investors to counteract the otherwise adverse market reaction. Consistent with these arguments, we find that conference calls are more likely for stock-for-stock mergers. We also expect acquirers' incentives to hold conference calls at merger announcements to be more pronounced for large and complex mergers where management's intent is more likely to be unclear to investors. Accordingly, we find that conference calls are more likely as the economic importance of the deal (as proxied by the deal value as a percentage of the acquirer's pre-announcement market value) increases.

We then examine the relation between management's decision to hold a conference call at the merger announcement and the initial market reaction to the announcement. After correcting for self-selection bias associated with the decision to hold merger-related conference calls using the inverse Mills' ratio, we find that bidders that hold conference calls at merger announcements experience announcement returns that are substantially higher than they would have experienced otherwise. We provide additional insights through a content analysis of merger-related information releases for a subsample of 20 transactions that are accompanied by conference calls and a matched sample of 20 economically similar transactions that are not accompanied by conference calls. This analysis indicates that the more favorable reaction is related to the fact that conference calls, compared to press releases, provide a greater volume of information and place greater emphasis on forward-looking details. We find no evidence that the superior announcement return subsequently reverses. Moreover, we find that stock-for-stock acquirers that hold conference calls concurrent with their merger announcements report less positive pre-announcement abnormal accruals than stock-for-stock acquirers that do not hold conference calls. This finding, which is consistent with Jo and Kim's (2007) evidence of a negative association between abnormal accruals and disclosure frequency prior to seasoned equity offerings (SEOs), indicates that acquirers' use of conference calls is not positively associated with opportunistic earnings management. Overall, the evidence suggests that managers use conference calls around merger announcements to credibly convey genuine favorable private information to the market as opposed to hyping their stock prices.

Our study extends the empirical voluntary disclosure literature by demonstrating the usefulness of voluntary disclosure generally and conference calls specifically outside the context of routine accounting events. Traditionally, the empirical voluntary disclosure literature focuses on disclosure activity linked to predictable accounting releases, such as management forecasts made in advance of earnings announcements (e.g., Baginski and Hassell 1997) and expanded press releases and clarifying conference calls made at the time of earnings announcements (e.g., Francis et al. 2002; Bowen et al. 2002; Kimbrough 2005). By contrast, our research highlights the potential usefulness of voluntary disclosure in the context of a (relatively) infrequent but potentially important strategic decision. In this regard, our work is related to Leone et al. (2007), who link managers' disclosure decisions about the intended use of initial public offerings (IPOs) proceeds to the degree of IPO underpricing.

Our findings also add to the literature on how bidders respond to the incentives to maximize share prices around mergers. Other studies in this area have focused on pre-announcement strategies. Brockman and Martin (2009) suggest that firms engaging in stock-for-stock mergers issue optimistic earnings forecasts prior to the merger announcements. Erickson and Wang (1999) and Louis (2004) focus on pre-merger announcement earnings management. These pre-announcement strategies are likely to favorably influence an acquirer's share price *prior* to a merger announcement. However, the benefit bidders receive from pre-announcement earnings guidance or earnings management is limited because, as Erickson and Wang (1999) argue, upon the announcement of a stock-for-stock merger, rational investors are likely to understand the prior incentives of the bidding firm's managers and, therefore, are likely to attempt to reverse the portion of the pre-announcement stock valuation attributable to misleading earnings guidance and earnings management. In contrast to the extant studies, we focus on concurrent disclosure because a stock-for-stock acquirer must be particularly concerned with the market response to the merger proposal and the ensuing valuation effect, which ultimately determines the cost and the likelihood of successful completion of the merger.

Our study is also related to Lang and Lundholm (2000) and Jo and Kim (2007). Using a 1992 sample of 41 firms with an average market capitalization of less than \$84 million, Lang and Lundholm (2000) document an increase in disclosure six months prior to SEOs, but find no evidence that managers provide more forward-looking information prior to the SEOs. In addition, they find that firms that increase their pre-SEO disclosure activity also experience positive pre-SEO abnormal stock returns, but suffer much larger price declines at, and over the months after, the SEO announcement than a set of control firms. Jo and Kim (2007) also find that firms with market capitalization of less than \$150 million (between 1990 and 1997) and an "unsustained" increase in disclosure inflate earnings aggressively prior to SEOs and have more negative performance over the long run. These results are consistent with managers acting opportunistically by using voluntary disclosure to hype their stock prices prior to SEOs.[1]

Our study differs from Lang and Lundholm (2000) and Jo and Kim (2007) in that we examine managers' contemporaneous disclosures made specifically about the economic event for which the stock is being used. Also, while these studies focus on small firms, we focus on a broader sample with an average market capitalization of more than \$2 billion. Moreover, the interactive nature of conference calls, as opposed to the static nature of press releases examined by Lang and Lundholm (2000) and Jo and Kim (2007), reduces managers' ability to mislead investors and limits potential misinterpretations by investors by forcing managers to respond to analyst inquiries.[2] Accordingly, in contrast to the extant studies, we find that: (1) acquirers provide substantial amounts of forward-looking information during merger-related conference calls, (2) acquirers that hold conference calls experience superior announcement returns, and (3) there is no evidence that the superior returns subsequently reverse. Thus, in contrast to Lang and Lundholm's (2000) results, our findings suggest that managers use contemporaneous disclosures made specifically about the economic event for which the stock is being used to convey genuine favorable private information.[3]

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[1] We do not examine stock issuances because the Security Data Company (SDC) provides filing dates of stock issuances but not the announcement dates. An analysis of about 50 issuance announcements reveals that the two dates are often different. In addition, the existence of cash-financed mergers provides a natural control sample, a feature that does not exist in the SEO setting.

Section II discusses managers' motivation for holding conference calls at the merger announcement. Section III describes the sample. Section IV analyzes the determinants of merger-related conference calls. Section V analyzes the capital market effects of merger-related conference calls. Section VI analyzes whether acquirers use conference calls to hype their stock prices. Section VII analyzes the content of the press releases and conference call transcripts for selected transactions. Section VIII concludes.

II. THE MOTIVATION FOR HOLDING MERGER-RELATED CONFERENCE CALLS

The Incentives for Additional Disclosure at Merger Announcements

To the extent that a proposed merger represents a substantial change in business strategy, historical information is unlikely to satisfy investors' current demands for information. As Dye (1985) and Jung and Kwon (1988) argue, when the market is aware that managers have private information and managers choose not to share that information, the market infers that the undisclosed information is unfavorable. Because managers have private information about the rationales for proposed mergers and their intended benefits, their disclosure decisions could have a substantial impact on investors' reactions to merger announcements.

According to Lipin and Sirower (2003), an effective merger communication strategy: (1) provides a credible story with clear targets that can be communicated and accomplished by the bidder and monitored over time by investors, (2) removes uncertainty and provides direction to the organization, and (3) links post-merger integration plans to the economics of the transaction. They point to PepsiCo's successful communication strategy in relation to its $13.4 billion purchase of Quaker Oats in December 2001. They particularly note that "PepsiCo got off to a good start with a detailed press release and investor presentation, *supported by a lengthy analyst/investor call and a Web cast*" (Lipin and Sirower 2003, 24; emphasis added).

Although all bidders have incentives to provide supplementary disclosure due to the well-documented cost of capital and reputational benefits of forthcoming disclosure, the incentives are particularly strong for economically significant deals where the intensity of investor demand for supplemental information is pronounced. In addition, disclosure incentives are stronger for the average stock-for-stock bidder because of the direct impact of its stock price on a merger's cost and likelihood of completion[4] and because, absent additional information, investors generally view the use of stock as a signal that managers have adverse private information (Travlos 1987).

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[2] Our study does not depend upon an elaborate theory of which investor groups are targeted by conference calls. We presume that conference calls are used to favorably influence the collective response of investors to the merger announcement. Regulation Fair Disclosure was in effect for our entire sample; therefore, the information conveyed in conference calls is not limited to a subset of investors.

[3] While we attribute the contrasting findings between our study and Lang and Lundholm's (2000) to differences between the two studies in the types of disclosures examined, it is important to note that gun-jumping laws that prevented disclosure of forward-looking information prior to the filing of a registration statement were in effect during the period studied by Lang and Lundholm (2000), whereas the Securities and Exchange Commission (SEC) has provided safe harbors for the ongoing release of forward-looking information during our sample period. However, this difference in regulatory regime is unlikely to fully account for the different results.

Conference Calls versus Alternative Communication Means

Conference calls allow managers to increase the volume of information transmitted at the merger announcement and communicate in an interactive manner with numerous analysts at one time. Mayew and Venkatachalam (2009) also suggest that managers' affect, as reflected in their voice patterns, can provide useful cues about future performance and that investors at least partially respond to the cues provided by managerial voice patterns during conference calls. Therefore, conference calls provide an ideal forum for managers to communicate important merger details, such as the anticipated benefits of the merger and projections of merger-induced financial performance. Our focus on conference calls as our indicator of supplemental disclosure is based on past research that documents their informativeness to investors (e.g., Tasker 1998; Bowen et al. 2002; Kimbrough 2005). In Section VII, we perform a detailed content analysis of merger announcement press releases and conference call transcripts.

Firms typically announce their acquisitions through press releases. However, given the contextual nature of the information that must be disclosed as part of an effective communication strategy, it is unlikely that a merger press release will be sufficient for large and complex mergers, particularly when management intent is unclear. Acquirers also communicate through SEC filings; however, given the extant evidence that investors do not process SEC filings in a timely manner,[5] it is unlikely that managers would rely mainly on these filings to counteract the potential adverse signal conveyed by the announcement of a stock-for-stock merger. Acquirers also communicate through their websites. However, none of these alternative communication vehicles offers substitutes for the interactive feature of conference calls.

Acquirers may also engage in road show presentations subsequent to the announcement of a proposed transaction. If bidders that use conference calls also engage in road show disclosures, then it is possible that conference calls do not completely reflect the information (incremental to the press release) conveyed by management to investors. However, because we focus on managers' disclosure choices and investors' responses during the narrow merger announcement period, the existence of road shows is unlikely to affect our study's inferences insofar as road shows typically take place after the initial merger announcement period.[6] Furthermore, Regulation Fair Disclosure (Reg-FD) was in effect during our entire sample period.[7] Any bidder in our sample that engaged in road show presentations would have had to disclose in advance to the public, through a press release or through a previously announced conference call, any material information that it planned to convey during the presentations. Our content analysis of press releases and conference calls in Section VII indicates that conference calls are the most likely forum for the release of information that management plans to subsequently disclose in road show presentations. Therefore, it seems reasonable to assume that conference calls capture the material information subsequently disclosed in road shows.

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[4] Under fixed value stock-for-stock arrangements, the value of the consideration target shareholders receive is certain but the number of shares the target shareholders will receive depends on the bidder's pre-closing stock price. In this case, investor reaction to the merger announcement has a direct impact on the number of shares that the bidder must issue, thereby determining the amount of dilution of existing shareholders' voting power and share of profits. Under fixed exchange stock-for-stock arrangements, the number of bidding firm shares that the target shareholders will receive is certain but the value of those shares is uncertain. In this situation, the target shareholders face greater uncertainty about the value of the consideration they are receiving and are less likely to agree to the merger if the bidder's share price drops after the merger is announced. Also, many stock-based mergers have collar provisions allowing the proposed merger to be cancelled if the bidders' stock price falls below a certain range.

[5] In general, prior studies find no evidence that the market reacts to the information in the 8-K filings in a timely manner (Pastena 1979; Fried and Schiff 1981; Johnson and Lys 1990; Klock 1994; Schwartz and Soo 1995). Extant studies also find limited or no reaction to new information disclosed in 10-Q and 10-K filings (Foster et al. 1983; Cready and Mynatt 1991; Stice 1991; Easton and Zmijewski 1993; Chung et al. 2003; Griffin 2003; Hollie et al. 2005; Li and Ramesh 2009).

[6] As discussed in Section IV, we focus on the initial merger announcement period based on the expectation that the incentive to engage in disclosure to favorably affect the market's reaction is likely to be most pronounced at the time of the initial merger announcement.

[7] The SEC has strongly suggested that companies take the following steps when publicly disclosing material information (earnings, for example) in order to comply with Reg-FD. "First, issue a press release, distributed through regular channels, containing the information; Second, provide adequate notice, by a press release and/or website posting, of a

III. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

The study covers merger bids on Security Data Company's (SDC) online database of domestic mergers and acquisitions that were announced between January 1, 2002 and December 31, 2006. A transaction is included in the sample if it satisfies the following criteria: (1) the bidder is a publicly traded company; (2) the transaction value, the method of payment, and the merger announcement date are contained in SDC; (3) the bidder has necessary data on the CRSP/Compustat merged database to compute market capitalization, leverage, and book-to-market ratio; (4) the transaction value is at least 10 percent of the bidder's market value; (5) the merger announcement press release could be located in Factiva or on the bidder's website; and (6) the transaction was not completed at the time of the announcement.

Because hand-collection and review of merger announcement press releases is costly, we impose a minimum deal ratio requirement (i.e., requirement 4) and focus our efforts on economically important mergers, where the role for voluntary disclosure is likely to be particularly strong. We require access to the merger announcement press release (i.e., requirement 5) for several reasons. First, we refer to the press release to confirm key transactional data obtained from SDC on the method of payment and the total transaction value. Second, and more importantly, we rely on the press release to identify the timing of a conference call relative to the merger announcement. Our sample period is subsequent to the passage of Reg-FD, which prohibits selective disclosure of material information. U.S. firms have generally complied with this requirement by making their conference calls open to the public and by announcing upcoming conference calls in their press releases. Therefore, we examine the merger announcement press release for any announcement of merger-related conference calls. Given that our focus is on how firms use voluntary disclosure to impact the costliness of proposed transactions, we exclude any transaction that is already completed at the time of announcement—i.e., where the announcement is on or after the completion date (requirement 6).

The selection process, which we summarize in Table 1, results in a sample of 1,228 transactions. We report descriptive statistics for the full sample as well as for the conference call and non-conference call subsamples in Table 2. Merger-related conference calls occur for about 62 percent (758/1,228) of the sample transactions. A number of notable and statistically significant differences between conference call and non-conference call transactions are apparent. Consistent with prior research, bidders that hold conference calls are substantially larger, have smaller book-to-market ratios (*BM*), are more heavily represented in high-technology industries, and have greater analyst following and institutional ownership than firms that do not. The deal ratio is also larger for mergers involving conference calls, suggesting that bidders are more likely to engage in conference calls as the economic importance of the transaction increases. Bidders that hold conference calls finance a larger proportion of their mergers with stock, providing a preliminary

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scheduled conference call to discuss the announced results, giving investors both the time and date of the conference call, and instructions on how to access the call; and Third, hold the conference call in an open manner, permitting investors to listen in either by telephonic means or through Internet webcasting" (SEC 2000, paragraph 4). The SEC has explicitly made road show presentations subject to Reg-FD requirements.

TABLE 1
Sample Selection Procedures

	Number of Bids
Total Merger or Acquisition transactions listed on the SDC database involving public bidders with non-missing data on transaction values, method of payment, and merger announcement dates and that were announced from January 1, 2002–December 31, 2006	12,840
Bidders without required variables on the CRSP/Compustat merged database	(4,602)
Transactions with a deal ratio of less than 10%	(6,186)
Duplicate transactions	(76)
Transactions where press releases could not be located in Factiva or on the bidding company's website	(539)
Announcements of already completed transactions	(209)
Final Sample	1,228

indication that disclosure incentives increase with the amount of the bidder's stock used to finance the transactions.[8]

Table 3 documents correlations. The correlations between conference calls and the remaining variables are consistent with the significant differences between the conference call and non-conference call subsamples highlighted in Table 2.

IV. DETERMINANTS OF MERGER-RELATED CONFERENCE CALLS

We analyze the determinants of firms' choice to hold conference calls at the merger announcement, using the following probit model:

$$\begin{aligned} CCALL_i = {} & \alpha_0 + \alpha_1 PCTSTOCK_i + \alpha_2 DEALRATIO_i + \alpha_3 INDR_i + \alpha_4 PRIV_i + \alpha_5 FOREIGN_i \\ & + \alpha_6 EARNANN_i + \alpha_7 LOGSIZE_i + \alpha_8 BM_i + \alpha_9 LOGANALYSTS + \alpha_{10} INSTOWN \\ & + \alpha_{11} REGULATED_i + \alpha_{12} HITECH_i + \alpha_{13} FINANCIAL_i + \text{yearly fixed effects} + \varepsilon_i \end{aligned} \quad (1)$$

where:

CCALL = 1 for bidders that hold merger-related conference calls on the day of or the day after the merger announcement, and 0 for other bidders;
PCTSTOCK = the value of stock consideration as a proportion of total consideration;
DEALRATIO = the ratio of the total transaction value to the bidder's pre-announcement market value as of the beginning of the fiscal year;
INDR = 1 when the bidder and the target have the same two-digit SIC code, and 0 otherwise;
PRIV = 1 when the target firm is a private company, and 0 otherwise;
FOREIGN = 1 when the target firm is a foreign company, and 0 otherwise;

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[8] The 25th (75th) percentile of *PCTSTOCK* for the full sample is 0(67) percent. For the conference call subsample, the 25th (75th) percentile of *PCTSTOCK* is 0(50) percent, while for the conference call subsample, the 25th (75th) percentile of *PCTSTOCK* is 0(67) percent.

TABLE 2
Descriptive Statistics

	Full Sample		Conference Call (CCALL = 1)		Non-Conference Call (CCALL = 0)			
	(n = 1,228)		(n = 758)		(n = 470)			
Variable	**Mean**	**Median**	**Mean**	**Median**	**Mean**	**Median**	**p-value for Mean Difference**	**p-value for Median Difference**
MKTCAP	2,083	443	2,979	723	636	224	<0.001	<0.001
PCTSTOCK	0.322	0.000	0.363	0.116	0.256	0.000	<0.001	<0.001
DEALRATIO	0.590	0.291	0.632	0.350	0.523	0.229	0.029	<0.001
BM	0.651	0.526	0.577	0.493	0.770	0.589	<0.001	<0.001
INDR	0.366	0.000	0.346	0.000	0.398	0.000	0.065	0.065
PRIV	0.346	0.000	0.299	0.000	0.421	0.000	<0.001	<0.001
FOREIGN	0.108	0.000	0.120	0.000	0.087	0.000	0.071	0.071
EARNANN	0.090	0.000	0.124	0.000	0.034	0.000	<0.001	<0.001
REGULATED	0.048	0.000	0.051	0.000	0.043	0.000	0.479	0.479
HITECH	0.352	0.000	0.392	0.000	0.287	0.000	<0.001	<0.001
FINANCIAL	0.232	0.000	0.191	0.000	0.298	0.000	<0.001	<0.001
ANALYSTS	5.343	4.000	6.776	5.000	3.031	2.000	<0.001	<0.001
INSTOWN	0.542	0.589	0.616	0.651	0.424	0.386	<0.001	<0.001

The p-values are two-tailed. The tests of mean differences are based on the t-statistic, assuming unequal variances, and the tests for median differences are based on the Wilcoxon rank sums statistic.

Variable Definitions:
CCALL = 1 for bidders that hold merger-related conference calls on the day of or the day after the merger announcement, and 0 for other bidders;
MKTCAP = market value of equity at the beginning of the fiscal year in which the merger is announced (expressed in millions);
PCTSTOCK = value of stock consideration as a proportion of total consideration;
DEALRATIO = ratio of the total transaction value to the bidder's pre-announcement market value as of the beginning of the fiscal year;
BM = ratio of the bidder's book value of equity to its market value of equity as of the beginning of the fiscal year in which the merger is announced;
INDR = 1 when the bidder and target have the same two-digit SIC code, and 0 otherwise;
PRIV = 1 when the target firm is a private company, and 0 otherwise;
FOREIGN = 1 when the target firm is a foreign company, and 0 otherwise;
EARNANN = 1 when the merger announcement date is within five days of a quarterly earnings announcement date, and 0 otherwise;
REGULATED = 1 for firms with two-digit SIC codes of 48 or 49, and 0 otherwise;
HITECH = 1 for firms with two-digit SIC codes equal to 28, 35, 36, 73, or 87, and 0 otherwise;
FINANCIAL = 1 for firms with two-digit SIC codes in the range of 60-69, and 0 otherwise;
ANALYSTS = number of analysts issuing earnings forecasts for the fiscal quarter in which the merger is announced; and
INSTOWN = percentage of the acquirer's stock held by institutional investors as of the beginning of the quarter in which the merger is announced.

TABLE 3
Correlations

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
(1) *CCALL*		**0.35**	**0.14**	**0.16**	**−0.17**	**−0.05**	**−0.12**	**0.05**	**0.15**	0.02	**0.11**	**−0.12**	**0.37**	**0.28**
(2) *MKTCAP*	**0.15**		**−0.06**	**−0.21**	**−0.31**	−0.04	**−0.29**	0.03	−0.02	0.03	**−0.13**	**0.06**	**0.69**	**0.55**
(3) *PCTSTOCK*	**0.13**	**0.13**		**0.23**	**−0.09**	**−0.07**	**−0.07**	**−0.07**	−0.01	−0.02	**0.08**	**0.12**	0.04	**−0.18**
(4) *DEALRATIO*	**0.06**	−0.05	**0.19**		**0.15**	0.03	**−0.12**	−0.01	0.00	**0.05**	**−0.07**	**−0.06**	**−0.21**	**−0.09**
(5) *BM*	**−0.17**	**−0.10**	**−0.06**	**0.21**		0.00	−0.02	−0.04	−0.01	**0.07**	**−0.17**	**0.08**	**−0.24**	**−0.14**
(6) *INDR*	**−0.05**	0.02	**−0.07**	**0.06**	−0.04		0.03	−0.01	**0.06**	**−0.08**	−0.04	0.00	**−0.09**	0.00
(7) *PRIV*	**−0.12**	**−0.15**	**−0.10**	−0.03	0.00	0.03		**−0.05**	0.04	−0.02	**0.06**	−0.01	**−0.19**	**−0.11**
(8) *FOREIGN*	**0.05**	−0.02	**−0.06**	−0.04	−0.04	−0.01	**−0.05**		0.00	−0.02	**0.15**	**−0.16**	0.03	0.02
(9) *EARNANN*	**0.15**	0.02	−0.01	0.01	−0.03	**0.06**	0.04	0.00		0.01	**0.09**	**−0.05**	−0.01	0.03
(10) *REGULATED*	0.02	**0.07**	−0.02	**0.10**	**0.12**	**−0.08**	−0.02	−0.02	0.01		**−0.17**	**−0.12**	−0.01	−0.04
(11) *HITECH*	**0.11**	**−0.07**	**0.08**	−0.05	**−0.10**	−0.04	**0.06**	**0.15**	**0.09**	**−0.17**		**−0.41**	**0.06**	0.03
(12) *FINANCIAL*	**−0.12**	0.04	**0.12**	**−0.05**	−0.01	0.00	−0.01	**−0.16**	**−0.05**	**−0.12**	**−0.41**		**−0.12**	**−0.19**
(13) *ANALYSTS*	**0.33**	**0.40**	**0.12**	**−0.15**	**−0.19**	**−0.06**	**−0.17**	0.00	−0.01	0.00	**0.07**	**−0.10**		**0.56**
(14) *INSTOWN*	**0.29**	**0.11**	**−0.19**	**−0.14**	**−0.12**	0.00	**−0.11**	0.03	0.03	−0.04	0.04	**−0.19**	**0.46**	

Spearman (Pearson) correlation coefficients are presented above (below) the diagonal. The coefficients in bold are all statistically significant at less than the 10 percent level in two-tailed tests.

See Table 2 for variable definitions.

EARNANN = 1 when the merger announcement date is within five days of a quarterly earnings announcement date, and 0 otherwise;
LOGSIZE = the log of the market value of equity at the beginning of the fiscal year in which the merger is announced;
BM = the ratio of the bidder's book value of equity to its market value of equity as of the beginning of the fiscal year in which the merger is announced;
LOGANALYSTS = the log of the number of analysts issuing earnings forecasts for the fiscal quarter in which the merger is announced;
INSTOWN = the percentage of the acquirer's stock held by institutional investors as of the beginning of the quarter in which the merger is announced;
REGULATED = 1 for firms with two-digit SIC codes of 48 or 49, and 0 otherwise;
HITECH = 1 for firms with two-digit SIC codes of 28, 35, 36, 73, or 87, and 0 otherwise; and
FINANCIAL = 1 for firms with two-digit SIC codes in the range of 60–69, and 0 otherwise.

Although firms can engage in disclosure to influence stock price anytime between the announcement of the merger and its completion, we use the narrow event window consisting of day 0 and day 1 relative to the merger announcement when determining the value of *CCALL*. Our use of a narrow event window is based on the conventional wisdom that it is difficult to reverse initially negative market reactions to merger announcements. As Lipin and Sirower (2003, 23) state, "many companies have discovered [that] it's hard to put the genie back in the bottle once a deal gets a bad reception." Therefore, we expect bidders' incentives to engage in disclosure to favorably affect the market's response to a merger to be most pronounced at the time of the initial merger announcement.

We determine whether an acquirer held a conference call on the day of or the day after the merger announcement (i.e., whether *CCALL* is equal to 0 or 1) based first on whether the merger announcement press release indicated plans to hold a conference call during this interval. In cases where the merger announcement press release did not contain a conference call announcement, we searched all press releases issued by the acquirer for up to 14 days following the merger announcement for evidence of a separate conference call announcement.[9]

Our primary interest is on the effect of the economic significance of the deal (*DEALRATIO*) and of the method of payment (*PCTSTOCK*). However, we control for other merger characteristics that are likely to influence the degree of information asymmetry associated with the merger and the resulting intensity of investor demand for supplemental information. These factors include the degree of industry relatedness (*INDR*) between the merging entities, the target's foreign status (*FOREIGN*), and the target's private status (*PRIV*).

We also control for previously documented determinants of conference call use. Specifically, we control for *EARNANN* because it is common for U.S. companies to hold conference calls in connection with earnings announcements. Prior research has documented a positive association between firm size and the extent of voluntary disclosure as a result of greater investor demand for disclosure and greater economies of scale in disclosure (Kasznik and Lev 1995). Past conference call research also documents that the decision to hold a conference call is decreasing in *BM* (Frankel et al. 1999). Analysts and institutional investors are likely to require more information from bidders and, because of Reg-FD, bidders can no longer provide information in private to them. Therefore, bidders' decisions to hold conference calls are likely to be influenced by analyst coverage and institutional ownership. Because the propensity to engage in a conference call likely varies across industries, we control for industry effects by incorporating separate indicator variables corresponding to the acquirer's membership in either high-tech, regulated, or financial industry categories. Finally, because companies' use of conference calls has increased dramatically over time, we control for year effects by including separate indicator variables corresponding to each of the announcement years represented in the sample.

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[9] We identify 12 transactions where the acquirers held merger-related conference calls outside of our narrow merger announcement window. We set *CCALL* equal to 0 for these transactions because we are interested in the effect of the calls made at the time of the merger announcement on the initial market reaction to the merger announcement. Conference calls held after the merger announcement cannot affect the initial market reaction. However, none of our inferences change if we instead set *CCALL* equal to 1 for these 12 transactions.

Table 4 presents the results of the probit estimation of Equation (1). Conference calls are more likely for large acquirers and high-technology firms. The likelihood of a merger-related conference call also increases with analyst coverage and institutional holdings and decreases with book-to-market. More importantly, we find that bidders are more likely to hold conference calls in connection with the announcement of proposed mergers as the relative size of the deal (*DEALRATIO*) increases ($p < 0.001$) and as the proportion of consideration to be paid in the form of stock (*PCTSTOCK*) increases ($p < 0.001$).[10] The latter finding is consistent with our conjecture that stock-for-stock bidders are likely to respond to their greater need to favorably influence investor reactions by providing expanded disclosure in the form of conference calls at the time of the merger announcement.[11]

A potential alternative explanation for this finding is that stock is often used in large transactions, which likely require more communication. Our analysis controls for acquirer size (*LOGSIZE*) and the relative size of the transactions (*DEALRATIO*). To further control for the potential effect of transaction size, we sort the value of the transactions into quartiles and compare the rate of conference call use for stock transactions (i.e., those transactions where *PCTSTOCK* is 50 percent or greater) and cash transactions within each quartile. Untabulated results show that the proportion of stock payment increases with the size of the deal. However, within deals of similar size (i.e., within each quartile), the rate of conference call use is always significantly higher in stock deals than in cash deals.

V. THE EFFECT OF CONFERENCE CALLS ON THE MARKET REACTION TO MERGER ANNOUNCEMENTS

We next analyze whether the use of conference calls results in a more favorable investor reaction to merger announcements. To the extent that firms anticipate the market reaction to a

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[10] The mean and standard deviation of *PCTSTOCK* are 32.2 percent and 40.0 percent, respectively. Therefore, the results in Table 3 indicate that a one standard deviation increase of 40.0 percent in *PCTSTOCK* from the mean of 32.2 percent to 72.2 percent is associated with an increase of approximately 8.71 percent in the likelihood of a merger-related conference call. Because a *PCTSTOCK* of 0 is 0.805 standard deviations below the mean *PCTSTOCK* of 32.2 percent and the unconditional probability of a merger-related conference call is 61.7 percent (758 conference call observations/1,228 total observations), the conditional probability of a merger-related conference call for observations where *PCTSTOCK* is equal to 0 is approximately 54.7 percent ($0.617 - [0.0871 \times 0.805]$). Similarly, because a *PCTSTOCK* of 1 is approximately 1.695 standard deviations above the mean *PCTSTOCK* of 32.2 percent and the unconditional probability of a merger-related conference call is 61.7 percent, the conditional probability of a merger-related conference call for observations where *PCTSTOCK* is equal to 1 is approximately 76.5 percent ($0.617 + [0.0871 \times 1.695]$). Hence, bidders that seek to finance their business combinations entirely with stock are 39.8 percent ($[76.5/54.7] - 1$) more likely to hold a merger-related conference call than bidders that intend to finance their transactions entirely with cash.

[11] Equation (1) treats *PCTSTOCK* as exogenous. This specification is reasonable under the assumption that bidders arrive at the deal details based on the economics of the transaction, independent of the conference call decision. There is a possibility, however, that the conference call decision is a determinant of the method of payment. To address this potential reverse causation, we employ an instrumental variables approach where we model the percentage of stock used in the transaction as a function of exogenous determinants suggested by prior literature. We then use the predicted value from this model as an instrumental variable for *PCTSTOCK* in Equation (1). Untabulated results show the conclusion that the proportion of stock in a transaction has a positive effect on the use conference calls still holds. Specifically, the coefficient on the instrumental variable is significantly positive (p-value < 0.001).

TABLE 4

Determinants of Merger-Related Conference Calls: Probit Estimation

$$CCALL_i = \alpha_0 + \alpha_1 PCTSTOCK_i + \alpha_2 DEALRATIO_i + \alpha_3 INDR_i + \alpha_4 PRIV_i + \alpha_5 FOREIGN_i + \alpha_6 EARNANN_i + \alpha_7 LOGSIZE_i + \alpha_8 BM_i + \alpha_9 LOGNALYST + \alpha_{10} INSTOWN + \alpha_{11} REGULATED_i + \alpha_{12} HITECH_i + \alpha_{13} FINANCIAL_i + \textit{yearly fixed effects} + \varepsilon_i$$

Dependent Variable: *CCALL*	Predicted Sign	Coefficient Estimate	p-value	Effect of Increase in Variable
Intercept	?	−2.008	<0.001	—
Method of Payment				
PCTSTOCK	+	0.603	<0.001	8.71%
Transaction Characteristics				
DEALRATIO	+	0.299	<0.001	9.20%
INDR	?	−0.103	0.228	−3.97%
PRIV	?	−0.002	0.977	−0.10%
FOREIGN	?	0.098	0.463	3.65%
Other Factors				
EARNANN	+	0.880	<0.001	25.84%
LOGSIZE	+	0.246	<0.001	15.17%
BM	−	−0.150	0.040	−3.13%
LOGANALYST	+	0.267	<0.001	8.90%
INSTOWN	+	0.322	0.031	3.82%
REGULATED	?	0.101	0.616	3.77%
HITECH	?	0.235	0.019	8.49%
FINANCIAL	?	−0.283	0.014	−11.07%
Yearly Fixed Effects	?	not tabulated	not tabulated	not tabulated
Pseudo R^2			22.15%	
n			1,228	

The p-values are one-tailed for coefficients with predicted signs and two-tailed otherwise.

Estimates are based on probit estimation. For continuous (binary) variables, the "Effect of Increase in Variable" is the change in the probability that a firm holds a conference call when the indicated variable is increased by one standard deviation (1) when all other continuous variables are set to their mean and all other binary variables are set to 0.

LOGSIZE is the log of the market value of equity at the beginning of the fiscal year in which the merger is announced. *LOGANALYST* is the log of the number of analysts issuing earnings forecast for the fiscal quarter in which the merger is announced.

See Table 2 for the definitions of the other variables.

merger announcement when deciding whether to hold a conference call, the relation between conference call use and the observed market reaction to the merger announcement is endogenous. This endogeneity may bias against finding a benefit to conference calls in an ordinary least-squares framework, particularly if acquirers are more motivated to hold conference calls to counteract anticipated unfavorable initial investor responses to their merger announcements. To account for the endogenous nature of the relation between conference call use and the observed market reaction to merger announcements, we estimate the following model that incorporates the endogeneity correction set forth by Heckman (1979):[12]

$$\begin{aligned} CAR_i = {} & \beta_0 + \beta_1 CCALL_i + \beta_2 PCTSTOCK_i + \beta_3 PRIV_i + \beta_4 PCTSTOCK_i \times PRIV_i \\ & + \beta_5 DEALRATIO_i + \beta_6 INDR_i + \beta_7 FOREIGN_i + \beta_8 LOGSIZE_i + \beta_9 BM_i \\ & + \beta_{10} LOGANALYST_i + \beta_{11} INSTOWN_i + \beta_{12} LAMBDA_i \times CCALL_i + \beta_{13} LAMBDA_i \\ & \times (1 - CCALL_i) + \varepsilon_i \end{aligned} \tag{2}$$

where *CAR* is the cumulative market-adjusted return over the three-day period spanning day 0 to day +2, where day 0 is the merger announcement date; and *LAMBDA* is the inverse Mills' ratio calculated according to Heckman (1979) based on the probit estimation of Equation (1).

The coefficient on *CCALL* (β_1) captures the reward to acquirers for engaging in conference calls. We include *PCTSTOCK* in the model based on prior evidence that bidders' announcement returns are generally negative for stock-for-stock acquisitions and positive for cash acquisitions.[13] We include *PRIV* and interact it with *PCTSTOCK* based on the finding in Chang (1998) that the private status of a target mitigates the negative market reaction to stock-for-stock mergers. We include *DEALRATIO* as a control for the economic importance of the merger. Prior studies document that this variable is positively related to bidders' announcement returns (Asquith et al. 1983). We include *INDR* because Morck et al. (1990) and Maquieira et al. (1997) find that returns to bidding firms are lower when the merger is diversifying. We include *FOREIGN* to capture the possibility that foreign targets' lack of visibility increases investor uncertainty and skepticism, thereby leading to lower announcement returns.[14] We include *LOGSIZE* because Loderer and Martin (1997) find that size is negatively correlated with the bidders' merger announcement stock returns. We control for *BM* based on evidence that Tobin's Q and book-to-market are negatively associated with bidder returns (e.g., Lang et al. 1989; Servaes 1991; Gong et al. 2008a). Moeller et al. (2007) suggest that acquirers' abnormal returns are negatively related to information asymmetry and diversity of opinion. We proxy for these factors by the acquirers' analyst coverage (*LOGANALYST*) and proportion of institutional holdings (*INSTOWN*). Following Tucker (2007), we allow the coefficient on the inverse Mills' ratio to differ for conference call and non-conference call transactions.[15]

We include the industry membership and announcement year indicator variables in the selection model (Equation (1)) and not in the outcome model (in Equation (2)) because these variables are highly associated with the conference call decision (they are jointly significant in Equation (1) at $p = 0.001$) but have no theorized effect on announcement returns. There is little doubt that industry membership and announcement year indicators are exogenous. We therefore use them as instrumental variables in the selection model. An untabulated test of over-identifying restrictions recommended by Larcker and Rusticus (2010) fails to reject the null hypothesis of exogeneity.

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[12] We exclude merger announcements that coincide with earnings announcements in order to avoid the contaminating effects of other information.

[13] See, for example, Travlos (1987), Lang et al. (1989), Servaes (1991), Chang (1998), Fuller et al. (2002), Shleifer and Vishny (2003), Louis (2004), Moeller et al. (2005), Malmendier and Tate (2008).

[14] The home bias literature indicates that domestic investors lack visibility in foreign firms (e.g., Kang and Stulz 1997; Ahearne et al. 2004; Covrig et al. 2001; Suh 2001; Bradshaw et al. 2004).

[15] Our results are unchanged if we constrain the coefficient on the inverse Mills' ratio to be the same for conference call and non-conference call transactions.

Therefore, industry membership and announcement year indicators represent suitable instrumental variables for identifying the system of equations.

Table 5 presents the results on the effect of conference calls on the market reaction to merger announcements. The univariate results in Panel A show that the average merger announcement abnormal return is lower for firms that hold conference calls than for those that do not. This finding is not surprising given that the decision to hold a merger-related conference call is positively related to factors that are generally associated with negative merger announcement returns, stock financing in particular. That is, an acquirer is more likely to hold conference calls when the nature of the transaction is such that the market is likely to react negatively in the absence of supplementary disclosure.

The results in Table 5, Panel B show the association between conference calls and the market reactions to merger announcements, after controlling for the potential selection bias related to the decision to hold merger-related conference calls and other determinants of the market reaction. Many of the coefficients on the control variables are significant and their signs are largely consistent with expectations, lending validity to the model. Turning to the main result, bidders that provide conference calls concurrent with their merger announcements experience announcement period returns that are about 6.5 percent ($p = 0.016$) higher than they would otherwise experience.

Although our discussion in Section II explains why stock acquirers have greater motivations than cash acquirers to hold merger-related conference calls, it is not clear (and we offer no expectation about) whether the realized benefits to conference calls are similar for stock and cash acquirers. In an untabulated supplemental test, we address this question by augmenting Equation (2) with an interaction of *PCTSTOCK* and *CCALL*. Because *CCALL* is endogenous, an interaction involving *CCALL* is also endogenous, and simply including the interaction in the model without an appropriate correction could lead to inconsistent estimates (Wooldridge 2002). We, therefore, follow the procedure suggested by Wooldridge (2008) for interacting binary endogenous variables and exogenous regressors. The coefficient on the resulting interaction is insignificant, which indicates no difference in the benefit to conference calls for cash versus stock acquirers. This finding likely reflects the fact that, once managers have decided to hold a conference call, the benefit will not necessarily vary across cash and stock transactions since managers are likely to hold conference calls when the firm is most likely to benefit from the call, irrespective of the method of payment.

Given the evidence that bidders that provide conference calls concurrent with their merger announcements experience higher announcement period returns than they would otherwise experience, the question that naturally emerges is: "Why are all merger announcements not accompanied by conference calls?" While the majority (62 percent) of the transactions in the full sample is accompanied by conference calls, the rate of conference call use is substantially less than 100 percent. We offer several conjectures as to why this is the case. First, the decision to hold conference calls is the result of a cost-benefit analysis. The benefits are more likely to accrue to acquirers that would have been over-penalized by the market because of the complexities of their transactions, the signal associated with the nature of the transactions, or some other reasons. Although providing a conference call has the potential to increase investor confidence and induce a more favorable (or less adverse) market reaction, voluntary disclosure is also costly. Proprietary information might be revealed in the process. Forward-looking disclosures expose managers to reputation loss and potential litigation (Francis et al. 1994; Skinner 1997). In addition, conference calls are a risky form of disclosure because, if managers' responses to analyst inquiries during conference calls do not pass muster, investor confidence could be undermined rather than

TABLE 5

The Effect of Merger-Related Conference Calls on the Market Reaction to Merger Announcements

Panel A: Univariate Comparisons of Event-Period Returns (p-values in parentheses)

	Conference Call (n = 653)	Non-Conference Call (n = 471)	Difference
Mean	0.005	0.023	−0.018
	(0.181)	(<0.001)	(0.001)
Median	−0.003	0.008	−0.011
	(0.710)	(<0.001)	(0.009)

Panel B: Multivariate Analysis of Event-Period Returns

$$CAR_i = \beta_0 + \beta_1 CCALL_i + \beta_2 PCTSTOCK_i + \beta_3 PRIV_i + \beta_4 PCTSTOCK_i \times PRIV_i + \beta_5 DEALRATIO_i + \beta_6 INDR_i + \beta_7 FOREIGN_i + \beta_8 LOGSIZE_i + \beta_9 BM_i + \beta_{10} LOGANALYST_i + \beta_{11} INSTOWN_i + \beta_{12} LAMBDA_i \times CCALL_i + \beta_{13} LAMBDA_i \times (1 - CCALL_i) + \varepsilon_i.$$

Variables	Predicted Sign	Coefficient Estimate	p-value
Intercept	?	0.063	<0.001
CCALL	+	0.065	0.016
PCTSTOCK	−	−0.070	<0.001
PRIV	?	−0.006	0.374
PCTSTOCK × *PRIV*	+	0.077	<0.001
DEALRATIO	+	0.008	0.051
INDR	−	0.010	0.964
FOREIGN	−	−0.009	0.148
LOGSIZE	−	−0.009	0.001
BM	−	0.002	0.685
LOGANALYST	−	−0.011	0.022
INSTOWN	−	−0.029	0.008
LAMBDA × *CCALL*	?	−0.031	0.153
LAMBDA × (1 − *CCALL*)	?	−0.045	0.013
n		1,099	
Adjusted R^2		14.44%	

Panel C: Multivariate Analysis of Long-Run Returns

$$LTRETURNS_i = \beta_0 + \beta_1 CCALL_i + \beta_2 PCTSTOCK_i + \beta_3 PRIV_i + \beta_4 PCTSTOCK_i \times PRIV_i + \beta_5 DEALRATIO_i + \beta_6 INDR_i + \beta_7 FOREIGN_i + \beta_8 LOGSIZE_i + \beta_9 BM_i + \beta_{10} LOGANALYST_i + \beta_{11} INSTOWN_i + \beta_{12} LAMBDA_i \times CCALL_i + \beta_{13} LAMBDA_i \times (1 - CCALL_i) + \varepsilon_i.$$

Variables	Coefficient Estimate	p-value
Intercept	−0.084	0.338
CCALL	−0.136	0.419
PCTSTOCK	−0.037	0.466

(continued on next page)

Panel C: Multivariate Analysis of Long-Run Returns

$$\begin{aligned} LTRETURNS_i = {} & \beta_0 + \beta_1 CCALL_i + \beta_2 PCTSTOCK_i + \beta_3 PRIV_i + \beta_4 PCTSTOCK_i \times PRIV_i \\ & + \beta_5 DEALRATIO_i + \beta_6 INDR_i + \beta_7 FOREIGN_i + \beta_8 LOGSIZE_i + \beta_9 BM_i \\ & + \beta_{10} LOGANALYST_i + \beta_{11} INSTOWN_i + \beta_{12} LAMBDA_i \times CCALL_i \\ & + \beta_{13} LAMBDA_i \times (1 - CCALL_i) + \varepsilon_i. \end{aligned}$$

Variables	Coefficient Estimate	p-value
PRIV	−0.008	0.830
PCTSTOCK × *PRIV*	−0.012	0.879
DEALRATIO	0.029	0.320
INDR	−0.036	0.233
FOREIGN	−0.022	0.649
LOGSIZE	0.032	0.035
BM	0.027	0.422
LOGANALYST	−0.009	0.977
INSTOWN	−0.025	0.703
LAMBDA × *CCALL*	0.008	0.947
LAMBDA × (1 − *CCALL*)	0.133	0.189
n	1,045	
Adjusted R^2	1.43%	

The p-values are one-tailed for coefficients with predicted signs and two-tailed otherwise. In Panel A, the tests of mean differences are based on the t-statistic, assuming unequal variances, and the tests for the median differences are based on the Wilcoxon rank sums statistic.

CAR is the cumulative market-adjusted return over the three-day period spanning day 0 to day +2, where day 0 is the merger announcement date. *LTRETURNS* is one-year buy-and-hold abnormal return starting on the third day after the merger announcement using the match-firm approach suggested by Barber and Lyon (1997). *LAMBDA* is the inverse Mills' ratio calculated according to Heckman (1979) based on the probit estimation of Equation (1) in Table 4.

See Table 2 for remaining variable definitions.

enhanced.[16]

Second, conference calls are likely to be beneficial only to those acquirers that have favorable information to impart, in addition to the information in the press releases and the observable characteristics of the mergers. Hutton et al. (2003) provide evidence that the market reacts to good news disclosures only when they are augmented with sufficient detail to make the good news credible. Otherwise, the market views good news assertions by managers as "cheap talk." Hence, it appears that, to benefit from conference calls in the form of a more favorable market reaction, firms must have good news that they can support with sufficient detail to make the good news credible. Therefore, it seems reasonable to assume that bidders hold conference calls when they believe that they have favorable information that they can impart through this disclosure medium. In Section VII, we provide evidence on the type of information provided during conference calls that appears to drive the more favorable reaction.

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[16] For example, analysts reacted skeptically to AT&T's initial conference call in connection with its proposed purchase in 1998 of TCI. Of particular concern to analysts was the plausibility of AT&T's growth projections. The concern expressed by analysts was so great that AT&T felt compelled to hold a second conference call only days after its initial conference call. In the second call, AT&T discussed the technical details and anticipated costs of integrating AT&T's and TCI's technologies. However, analysts responded to the second conference call by continuing to question many aspects of the deal, particularly AT&T's assumptions. This case provides an example of a situation where providing additional details actually fuels rather than alleviates analyst and, by extension, investor skepticism. Highlighting this risk, we document in untabulated analysis of a matched sample that investor response to the announcement of stock-for-stock transactions is more favorable for the conference call transactions 62 percent of the time. While this represents a clear majority of the transactions, the 38 percent of the time where conference call transactions are greeted with less favorable reactions is nontrivial.

VI. DO ACQUIRERS USE CONFERENCE CALLS TO HYPE THEIR STOCK PRICES?

Acquirers that provide conference calls concurrent with their merger announcements could experience superior market performance at merger announcements because managers use the conference calls to hype their stock prices. We test this conjecture by analyzing the acquirers' post-merger announcement abnormal returns and pre-merger announcement abnormal accruals.

Post-Merger Announcement Abnormal Returns

If managers use conference calls to temporarily inflate their stock prices prior to the transactions, then the relatively favorable market reaction we document would be temporary. Therefore, we compare the acquirers' abnormal returns over the year after the merger announcement across acquirers that hold conference calls and those that do not. If the managers use the conference calls around merger announcements to temporarily hype their stock prices, then we expect the post-merger announcement abnormal returns to be more negative when the merger announcements are accompanied by conference calls.

We estimate a firm's long-term abnormal returns (*LTRETURN*) by adjusting its buy-and-hold return by the return of a matched firm, as suggested by Barber and Lyon (1997) and Kothari and Warner (1997). Following Barber and Lyon (1997), we choose the match firm from all firms with a market value of equity between 70 percent and 130 percent of the market value of equity of the acquirer as of the beginning of the quarter in which the merger was announced. From this set of firms, we match the acquirer with the firm that has the closest book-to-market ratio.

We then reestimate Equation (2) after replacing *CAR* with *LTRETURN*. The results, reported in Table 5, Panel C provide no evidence that firms that hold conference calls concurrent with their merger announcements have worse post-merger announcement market performance than those that do not hold conference calls (i.e., β_1 is insignificant). Therefore, there is no evidence that the more favorable market reaction to merger announcements that are accompanied with conference calls subsequently reverses, which suggests that, in general, managers use conference calls around merger announcements as a means to convey genuine private information as opposed to falsely hyping their stock prices.

Pre-Merger Announcement Abnormal Accruals

Prior studies also suggest that stock-for-stock acquirers tend to report higher pre-merger announcement abnormal accruals (Erickson and Wang 1999; Louis 2004). Louis (2004) also suggests that investors are generally misled by the pre-merger announcement abnormal accruals. Therefore, to further examine whether acquirers that provide conference calls concurrent with their merger announcements are motivated by the desire to hype their stock prices, we analyze the association between stock-for-stock acquirers' use of conference calls and the abnormal accruals they include in the earnings reported within 90 days prior to the merger announcement.[17] If acquirers that use conference calls around merger announcements are motivated by the desire to hype their stock prices, then these acquirers would presumably have a stronger motivation to report higher pre-merger announcement abnormal accruals.

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[17] We classify transactions as stock-for-stock if 50 percent or more of the consideration is comprised of the acquirer's stock. We use a 50 percent threshold to isolate transactions where the incentives associated with the use of stock are pronounced, while ensuring that we have enough observations to conduct our analysis. The average percentage of stock financing for our stock-for-stock subsample using this threshold is about 85 percent.

We measure abnormal accruals by the residual from a modified Jones (1991) model similar to the model in Louis and White (2007) and Gong et al. (2008a, 2008b). Specifically, for each calendar quarter and two-digit SIC-code industry, we estimate the following model using all firms that have the necessary data on Compustat:

$$TA_i = \sum_{j=1}^{4} \lambda_{j-1} Q_{j,i} + \lambda_4(\Delta SALE_i - \Delta AR_i) + \lambda_5 PPE_i + \lambda_6 LTA_i + \varepsilon_i \tag{3}$$

where:

TA = total accruals;
Q_j = a binary variable taking the value of 1 for *fiscal* quarter j, and 0 otherwise;
$\Delta SALE$ = the quarterly change in sales;
ΔAR = quarterly change in accounts receivable;
PPE = property, plant, and equipment at the beginning of the quarter; and
LTA = the fourth lag of total accruals.

We measure total accruals as earnings before extraordinary items and discontinued operations (Compustat quarterly data item #76) minus operating cash flows (#108 − #78). If a firm does not have operating cash flows on Compustat, then we compute total accruals as change in non-cash current assets (change in item #40 minus change in item #36) minus change in current liabilities (change in data item #49) plus change in debt in current liabilities (change in data item #45) minus depreciation (data item #5). All variables are scaled by total assets at the beginning of the quarter. To mitigate the effect of outliers and errors in the data, for each calendar quarter, we delete the top and bottom one-percentiles of the deflated TA, ($\Delta SALE - \Delta AR$), and PPE. We then delete the top and bottom one-percentiles of the regression residual (our abnormal accrual estimate). We also require at least 10 observations for each estimation.

Following Kothari et al. (2005), we adjust the estimated abnormal accruals for performance. For each quarter and each industry (two-digit SIC code), we create five portfolios by sorting the data into quintiles based on the return-on-assets from the same quarter in the previous year.[18] The performance-matched abnormal accruals for a sample firm are the firm-specific abnormal accruals minus the mean (excluding the sample firm) abnormal accruals for its respective industry-performance-matched portfolio.

Table 6 reports the abnormal accruals for the quarter prior to the merger announcement. Consistent with prior studies, the abnormal accruals are positive; however, they are substantially larger for those acquirers that do not hold conference calls at the time of the merger announcement. The difference in abnormal accruals between stock-for-stock acquirers that hold conference calls and those that do not is 0.0158 (two-tailed p-value of 0.047). One plausible explanation for this result is that stock-for-stock acquirers that inflate their accruals are more likely to refrain from holding conference calls to avoid questions about their financial reports. To provide evidence on this possibility, we also report differences in the pre-announcement accruals across high and low

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[18] Ideally, we would match on the current quarter's ROA. However, this is not practical because current ROA (i.e., the ROA for the quarter immediately before the merger announcement) is affected by the pre-merger announcement earnings management that we are trying to capture. Our matching procedure is consistent with Louis (2004), Fischer and Louis (2008), Louis and White (2007), Gong et al. (2008a, 2008b), and Louis et al. (2008), among others.

TABLE 6

Stock-for-Stock Acquirers' Average Abnormal Accruals Conditional on Conference Calls and the Level of Analyst Coverage

	(1) Non-Conference Call		(2) Conference Call		(2)−(1) Difference	
	Mean	**Median**	**Mean**	**Median**	**Mean**	**Median**
All	0.0218 (0.007) {n = 53}	0.0149 (0.008) {n = 53}	0.0060 (0.058) {n = 187}	0.0077 (0.027) {n = 187}	−0.0158 (0.047)	−0.0072 (0.047)
Low analyst coverage	0.0249 (0.009) {n = 35}	0.0125 (0.016) {n = 35}	0.0063 (0.116) {n = 102}	0.0085 (0.070) {n = 102}	−0.0186 (0.052)	−0.0040 (0.072)
High analyst coverage	0.0157 (0.169) {n = 18}	0.0161 (0.123) {n = 18}	0.0057 (0.155) {n = 85}	0.0050 (0.155) {n = 85}	−0.0100 (0.280)	−0.0110 (0.188)

Two-tailed p-values are reported in parentheses. The tests of mean differences are based on the t-statistic, assuming unequal variances, and the tests for median differences are based on the Wilcoxon rank sums statistic.

The abnormal accruals are for the quarter immediately preceding the merger announcement by stock-for-stock acquirers. Accruals are scaled by lagged total assets. The measurement process is described in Section VI.

analyst followings. We find no evidence that the lower magnitude of accruals for conference call transactions is more pronounced for the high analyst coverage subsample, which would be expected if acquirers that engage in accrual inflation seek to avoid the analyst scrutiny associated with conference calls. Thus, the results seem more consistent with the notion that acquirers that hold conference calls concurrent with stock issuances are motivated more by a desire to convey genuine private information than to falsely hype their stock prices.

VII. SMALL SAMPLE EVIDENCE ON THE INFORMATION CONTENT OF CONFERENCE CALLS

In Section V, we provide evidence that merger announcements accompanied by conference calls experience relatively more favorable market reactions and suggest that this finding is due to the information provided by management during these calls. In this section, we use content analysis on a subsample of transactions in order to provide direct evidence on the information content of conference calls. We then examine whether the information content of the conference calls contributes to the relatively more favorable response to merger announcements that are accompanied by conference calls. Because of the cost of coding conference call disclosures, we perform this content analysis on a subsample of transactions.

Identification and Characteristics of the Subsample

The subsample for the content analysis consists of those transactions where conference call transcripts were available either from FD Wires or StreetEvents and where a suitable control transaction could be located. The objective of our matching process is to identify transactions that were as economically similar as possible to the treatment transactions to maximize the ability to attribute differences in the information disclosed by the conference and non-conference call acquirers to managerial choice rather than to differences in the economics of the mergers.

We impose an initial requirement that the estimated likelihood of conference call use from estimating Equation (1) for the control transaction be within 20 percent of the corresponding conference call transaction.[19] For the subset of potential control transactions meeting this criterion, we impose the following additional criteria designed to make sure the final pairs have similar transaction characteristics: (1) the percentage of stock for the control transaction is within 5 percent of the percentage of stock for the conference call transaction; (2) the difference in deal ratio between the treatment and control transactions is less than 20 percent; (3) the percentage difference in acquirer size between the treatment and control transactions is less than 20 percent; and (4) the transaction takes place within a year of the treatment transaction.

In cases where we identify multiple control transactions meeting the above criteria for a given treatment transaction, we retain the control transaction that is nearest to the treatment transaction on the basis of the estimated probability of conference call use. This process leads to the identification of 147 treatment-control pairs of cash-financed mergers and 20 treatment-control pairs of stock-for-stock mergers.[20] Table 7 provides comparative descriptive statistics on the conference call transactions and their corresponding control transactions. The differences between the characteristics of the conference call and control transactions are insignificant, with the exception of the *DEALRATIO* in the case of the cash transactions. Thus, we believe that our matching procedures are successful in identifying control transactions that are economically similar to the conference call transactions.

Table 7 also compares *CAR* across the treatment and the control firms. The results reported in Table 7, Panel A provide no evidence that investors react more favorably to the announcement of a cash merger for the conference call group than for the non-conference call group. The mean (median) pairwise difference in *CAR* is -0.66 percent (-0.67 percent), with a one-tailed p-value of 0.843 (0.842). In contrast, the results reported in Panel B of Table 7 for the stock-for-stock mergers show that, on average, *CAR* is higher for the conference call transactions than for the control transactions. The mean (median) pairwise difference in *CAR* between conference call and non-conference call firms of 4.79 percent (3.90 percent) is significant with a one-tailed p-value of 0.014 (0.016), which indicates that investors react more favorably to the announcement of a stock-for-stock merger when the announcement is accompanied by a conference call.[21] Our failure to find evidence of benefits to conference calls for cash acquirers in the matched sample contrasts with the substantial benefits we document in Table 5 for the full sample and is likely attributable to differences between the matched sample and the full sample.[22] Since evidence of relatively more favorable market responses to conference call transactions is limited to stock transactions, we limit our detailed content analysis to those transactions.

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[19] The estimated probability of conference call use is a convenient matching criterion because it parsimoniously captures the factors relevant to the managers' conference call decisions. However, using it as the only match criterion results in significant pairwise differences on dimensions directly relevant to the economics of the transactions, such as: percentage of stock, deal ratio, and acquirer size. Therefore, we use the estimated probability as the first but not the only matching criterion.

[20] We obtain few matched pairs for the stock transactions because of the lack of mergers with similar acquirer size and deal ratio that were announced within a year of the announcement date for the conference call transactions.

[21] To ensure that our results are not due to a failure to adequately control for differences between the conference call and control transactions, we also regress the pairwise differences in *CAR* against pairwise differences in size, percentage of stock, deal ratio, book-to-market, analyst following, and the estimated probability of conference call use based on Equation (1). The conditional mean pairwise difference in *CAR* between conference-call and non-conference-call acquirers is captured by the regression intercept. The results from this multivariate analysis are consistent with our univariate analysis. Specifically, the conditional mean pairwise difference in *CAR* between the two groups of stock-for-stock acquirers is 5.03 percent (one-tailed p-value = 0.01). In contrast, for cash transactions, the mean pairwise difference is -1.23 percent (two-tailed p-value of 0.09).

[22] It is worth noting that the subsample differs from the full sample in terms of market capitalization (average market capitalization for this subsample of $625 million versus the full sample average of $2 billion). The average deal ratio from the subsample of 28 percent is also smaller than the average for the full sample of 59 percent. These differences reflect the difficulty in identifying matches for the largest firms and transactions in our sample for which comparable

Development of Taxonomy Used to Classify Merger-Related Disclosures

At the outset of the project, we initially read and transcribed all disclosures made in a small random selection of press releases and conference call transcripts. We then grouped the disclosures into similar categories based on the nature of the disclosure, which required judgment. Given this subjectivity of the classification process, we include an Appendix to provide transparency on our classification choices.

As indicated in the Appendix, we classify the disclosures from the initial transcription process into six major categories: (1) deal details, which outline the terms of the transaction; (2) acquirer business, which provides historical information on the acquirer's business; (3) target business, which provides historical information on the target's business; (4) strategic rationale, which outlines the strategic rationale for the merger; (5) integration details, which outline plans for post-merger consolidation of operations; and (6) financial guidance, which provides guidance on the financial impact of the merger. We characterize information from the first three categories as historical information and information from the last three categories as forward-looking information.

Coding Procedures

We applied the classification scheme set forth in the Appendix to the press releases and transcripts that comprise our matched sample. The process involved a line-by-line reading of the press release and/or conference call transcripts. For each sentence or related group of sentences, we determined the nature of the disclosure made, the number of words devoted to the disclosure, and referred to the classification scheme in the Appendix to determine the appropriate classification of the disclosure.[23]

When counting the number of words in the conference call transcripts, we ignored redundancies between the conference call and the press release disclosures because we are interested in the incremental information provided in the conference calls.[24] In addition, because we were only interested in the nature and amount of information provided by management, we included only management disclosures in the word count and exclude words attributable to analysts during the question-and-answer (Q&A) session of the conference calls.

Within-Transaction Comparisons

For the 20 stock transactions that are accompanied by conference calls, we examine the information content of the press releases and the accompanying conference calls. We measure the

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firms do not exist. Despite the differences, the subsample does exhibit the finding in the full sample that the conference call transactions have relatively more favorable reactions than the non-conference call transactions. Therefore, content analysis for this subsample is likely to provide insights on the drivers of the relatively more favorable reaction to conference call transactions that we document for the full sample.

[23] The coding process was admittedly subjective. However, in the course of coding, we encountered no disclosures that were not contained in the initial coding scheme set forth in the Appendix. Therefore, once the coding scheme was agreed upon, we had to exercise very little judgment about how disclosures should be classified. As a further check on the consistency of our coding, we hired a research assistant to independently code the press releases and conference call transcripts based on the coding scheme we provided. This research assistant was unaware of our expectations and had no exposure to any of the data. We found that our inferences were unchanged using the research assistant's coding.

[24] Redundancies are primarily from the following sources. In the prepared remarks section of the conference call, management's "script" often includes direct quotations from the press release. Even in cases where management does not recite direct passages from the press release, they often repeat basic details that are already contained in the press release such as the amount or form of consideration. There are also instances during the question-and-answer portion of the call when analysts requested that management repeat details disclosed earlier in the call. In all such cases, the coding author excluded the repeated disclosures when coding the information content of the conference call.

TABLE 7

Transaction and Bidder Attributes for a Sample of Merger Announcements Accompanied by Conference Calls and a Control Sample of Merger Announcements with Press Releases Only

Panel A: Cash Transactions (n = 147)

	Conference Call Sample	Control Sample	Pairwise Difference Conference Call − Control	p-value of Pairwise Difference
	Mean [Median]	Mean [Median]	Mean [Median]	Mean [Median]
MKTCAP (millions)	871	897	−26	0.075
	[466]	[472]	[−2]	[0.351]
PCTSTOCK	0.00	0.00	0.00	0.159
	[0.00]	[0.00]	[0.00]	[0.500]
DEALRATIO	0.28	0.25	0.03	<0.001
	[0.23]	[0.18]	[0.02]	[<0.001]
BM	0.625	0.670	−0.045	0.319
	[0.572]	[0.532]	[−0.043]	[0.546]
ANALYSTS	5.05	4.80	0.26	0.391
	[5.00]	[4.00]	[0.00]	[0.180]
PROBABILITY	54.60	52.50	2.10	0.143
	[55.05]	[53.60]	[0.44]	[0.133]
CAR (%)	1.30	1.96	−0.66	0.843
	[0.62]	[1.34]	[−0.67]	[0.842]
LTRETURNS (%)	−5.11	0.52	−5.63	0.073
	[−3.04]	[−5.05]	[−2.25]	[0.240]

Panel B: Stock-for-Stock Transactions (n = 20)

	Conference Call Sample	Control Sample	Pairwise Difference Conference Call − Control	p-value of Pairwise Difference
	Mean [Median]	Mean [Median]	Mean [Median]	Mean [Median]
MKTCAP (millions)	626	623	3	0.863
	[415]	[473]	[−7]	[0.596]
PCTSTOCK	0.92	0.91	0.01	0.028
	[1.00]	[1.00]	[0.00]	[0.063]
DEALRATIO	0.28	0.28	0.00	0.844
	[0.21]	[0.20]	[0.01]	[0.841]
BM	0.560	0.605	−0.045	0.571
	[0.492]	[0.523]	[−0.011]	[0.784]
ANALYSTS	5.65	4.90	0.75	0.487
	[4.00]	[4.00]	[0.00]	[0.692]
PROBABILITY	66.24	62.81	3.43	0.143
	[63.08]	[66.12]	[2.41]	[0.133]
CAR (%)	2.17	−2.62	4.79	0.014
	[0.27]	[−3.64]	[3.90]	[0.016]
LTRETURNS (%)	10.72	−10.76	21.48	0.355
	[−9.42]	[−8.29]	[5.27]	[0.546]

(continued on next page)

All p-values are two-tailed, except those related to *CAR*, which are one-tailed. The significance of mean pairwise differences is based on one-sample t-tests and the significance of median pairwise differences is based on the Wilcoxon sign-rank test.

PROBABILITY is the estimated probability of holding a conference call based on the probit estimation of Equation (1). The other variables are defined as in Tables 2, 4, and 5.

volume of information provided using word count. In untabulated analysis, we find that the conference call provides an average of 4.3 times more total volume of information than the press release ($p < 0.01$). Moreover, conference calls provide significantly greater volume of information across all information categories except for acquirer business and deal details. Table 8, Panel A summarizes within-transaction comparisons of the relative emphasis on information categories in merger announcement press releases versus conference call transcripts for the 20 treatment transactions. We calculate the relative emphasis devoted to a category as the proportion of the total word count attributable to that category. While only 45 percent of the discussion in press releases pertains to forward-looking details, over 70 percent of the disclosure volume in conference calls is devoted to forward-looking information.

We also examine differences in the information content of the prepared-remarks section of the conference call versus the Q&A session. A key feature of conference calls is the ability of call participants to probe management for additional details during a Q&A session that typically follows prepared remarks by management. Therefore, we are interested in how important these Q&A sessions are in eliciting additional information from management. Untabulated analysis indicates that the Q&A portion of conference calls has roughly equal volume to the prepared remarks portion (mean and median pairwise differences are insignificant). Thus, the opportunity conference calls offer to analysts to probe management appears to be a major source of the total information provided in these calls.[25]

Table 8, Panel B summarizes within-transaction comparisons of the relative emphasis on information categories in the prepared remarks portion of the conference call versus the Q&A portion. There is no significant difference in the overall emphasis placed on historical or forward-looking information in the Q&A session versus the press release. However, there is a greater emphasis on integration details during the Q&A portion, while there is a significantly greater discussion of the acquirer business and the strategic rationale for the transaction in the prepared remarks section.

Between-Transaction Comparisons

We next compare the total information provided by the treatment and control samples. The volume of information provided in the conference call transactions is the sum of the word counts contained in the press release and in the related conference call transcripts. We aggregate across these information sources because we are interested in comparing the overall volume of disclosure provided by conference call transactions to that provided in control transactions.[26] In untabulated

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[25] In contrast to our findings, Matsumoto et al. (2006) find, in the context of earnings announcement conference calls, that a greater proportion of the total call volume occurs during the Q&A portion of the conference call. These contrasting findings highlight the different insights that are gained by examining conference calls outside of the earnings contexts that have been the primary focus of prior studies.

[26] Recall that in computing word count totals for conference call transcripts, we avoid including any conference call disclosures that are redundant with disclosures made in the corresponding press release. Thus, double counting of disclosures is not an issue when aggregating word counts across the two information sources.

TABLE 8

Information Provided in Conference Calls and Press Releases for a Subsample of 20 Stock-for-Stock Merger Announcements Where the Bidders Provide Both Press Releases and Conference Calls

Panel A: Press Releases versus Conference Call: Within-Transaction Comparisons

	(1) Press Release		(2) Conference Call		(1)−(2) Pairwise Difference	
	Mean	Median	Mean	Median	Mean (p-value)	Median (p-value)
Deal details	27.91	26.47	9.02	8.48	18.89 (<0.001)	16.55 (<0.001)
Acquirer business	14.78	11.85	4.02	2.77	10.76 (0.001)	10.52 (0.001)
Target business	12.00	10.91	14.42	11.59	−2.41 (0.351)	1.06 (0.891)
Strategic rationale	32.95	33.18	43.91	42.71	−10.96 (0.006)	−8.96 (0.008)
Integration details	8.03	7.05	18.76	16.27	−10.73 (<0.001)	−9.83 (<0.001)
Financial guidance	4.32	2.65	9.87	10.18	−5.55 (0.006)	−5.21 (0.011)
Total forward-looking details	45.31	43.85	72.55	77.71	−27.24 (<0.001)	−28.13 (<0.001)

Panel B: Prepared Remarks versus Q&A Portion: Within-Transaction Comparisons

	(1) Prepared Remarks		(2) Q&A		(2)−(1) Pairwise Difference	
	Mean	Median	Mean	Median	Mean (p-value)	Median (p-value)
Deal details	9.56	8.82	8.52	4.13	−1.04 (0.695)	−0.39 (0.622)

(continued on next page)

Panel B: Prepared Remarks versus Q&A Portion: Within-Transaction Comparisons

	(1) Prepared Remarks		(2) Q&A		(2)−(1) Pairwise Difference	
	Mean	**Median**	**Mean**	**Median**	**Mean (p-value)**	**Median (p-value)**
Acquirer business	6.26	3.85	1.48	0.00	−4.79 (<0.001)	−3.24 (<0.001)
Target business	11.12	8.37	13.54	6.48	2.42 (0.557)	0.27 (0.709)
Strategic rationale	52.36	49.15	38.92	39.21	−13.44 (0.029)	−12.39 (0.019)
Integration details	12.86	11.12	24.71	20.85	11.85 (0.005)	10.89 (0.007)
Financial guidance	7.83	6.52	12.83	11.50	5.00 (0.097)	1.63 (0.133)
Total forward-looking details	73.05	74.45	76.47	80.33	3.41 (0.483)	6.53 (0.349)

All p-values are two-tailed.

The information categories are described in the Appendix. The significance of mean pair-wise differences is based on one-sample t-tests and the significance of median pair-wise differences is based on the Wilcoxon sign-rank test.

analysis, we find that the total volume of information for the conference call transactions is eight times as large as the total volume of information for the non-conference call transactions. Moreover, the significantly greater volume of disclosure in conference call transactions extends to all information categories.

Table 9 reports between-transaction comparisons of the relative emphasis on information categories in merger announcement press releases versus conference call transcripts. While roughly two-thirds of the disclosure volume in conference call transactions is devoted to forward-looking information, only one-third of the disclosure volume in control transactions is devoted to forward-looking information. Looking at individual categories, we find that control transactions emphasize information on deal details and the acquirer's business more than the conference call transactions, while conference call transactions emphasize discussion of the strategic rationale, integration details, and financial guidance more than the control transactions. Overall, the emphasis on forward-looking information in the conference call transactions is approximately 30 percent higher than that in the control transactions ($p < 0.001$ for the mean and median pairwise differences).

Association between Differences in Information Content and Differences in Market Reaction

The foregoing content analysis indicates that conference call transactions provide a greater volume of disclosure and emphasize forward-looking details to a greater degree. To determine

TABLE 9

Pairwise-Comparisons of the Relative Emphasis on Information Categories for the 20 Stock-for-Stock Merger Announcements Accompanied by Conferences and a Control Sample of 20 Stock-for-Stock Transactions with a Press Release Only

	(1) Conference Call Sample		(2) Control Sample		(1)−(2) Pairwise Difference	
	Mean	Median	Mean	Median	Mean (p-value)	Median (p-value)
Deal details	13.51	11.98	37.76	32.11	−24.45 (<0.001)	−17.46 (<0.001)
Acquirer business	7.04	5.96	13.85	13.94	−6.81 (<0.001)	−7.06 (<0.001)
Target business	13.51	11.77	14.31	12.03	0.80 (0.835)	0.50 (0.870)
Strategic rationale	40.69	38.48	24.71	26.07	15.98 (0.001)	15.51 (0.001)
Integration details	16.51	13.59	5.65	2.86	10.86 (0.001)	10.83 (0.001)
Financial guidance	8.73	8.51	3.71	2.40	5.02 (<0.001)	4.99 (<0.001)
Total forward-looking details	65.93	69.33	34.07	33.21	31.86 (<0.001)	30.63 (<0.001)

All p-values are two-tailed.

The information categories are described in the Appendix. The significance of mean pair-wise differences is based on one-sample t-tests and the significance of median pair-wise differences is based on the Wilcoxon sign-rank test.

whether these differences in information content are indeed driving the relatively more favorable response to conference call transactions, we use the following two alternative measures of information content:

DIFFWORDCOUNT = the ratio of the total word count provided in conference call transactions to the total word count provided in control transactions; and

DIFFFUTURE = the percentage of total words devoted to forward-looking details provided in conference call transactions minus the percentage of total words devoted to forward-looking details provided in the control transactions.

For each information content measure, we designate pairs in the upper (lower) half of the sample distribution as those where the amounts of additional information provided in the conference call transaction relative to the control transaction are large (small). We then examine pairwise differences in announcement period returns for each subsample. The results in Table 10 show that, for both information content measures, the relatively more favorable investor responses to conference call transactions is limited to those transactions where the information differential between the conference call transaction and the control transactions is large. This result provides some indication that the greater volume of details disclosed, as well as the greater emphasis on forward-looking information, contribute to the more favorable reactions observed for the conference call transactions.

VIII. CONCLUSION

We investigate the factors that influence whether bidders engage in voluntary disclosure at the time they announce a proposed transaction. We find that bidders are more likely to hold conference calls in connection with the announcement of proposed mergers as the proportion of consid-

TABLE 10

Analysis of Pairwise Differences in Market Reactions Based on Pairwise Differences in Information Content

Information Content Measure	Large Differences in Information Content	Small Differences in Information Content
DIFFWORDCOUNT	Mean: 8.81% (p = 0.034) Median: 7.41% (p = 0.049) {n = 10}	Mean: 0.77% (p = 0.476) Median: 0.00% (p = 0.625) {n = 10}
DIFFFUTURE	Mean: 6.94% (p = 0.034) Median: 5.05% (p = 0.027) {n = 10}	Mean: 2.65% (p = 0.382) Median: 0.85% (p = 0.625) {n = 10}

All p-values are two-tailed.

We rank our 20 pairs of conference call-control transactions on the basis of differences in information content (which we measure alternatively as *DIFFWORDCOUNT* and *DIFFFUTURE*). We designate those pairs that are above (below) the median as having large (small) differences in information content.

DIFFWORDCOUNT is the ratio of the total word count provided in conference call transactions to the total word count provided in control transactions. *DIFFFUTURE* is the percentage of total words devoted to forward-looking details provided in conference call transactions minus the percentage of total words devoted to forward-looking details provided in the control transactions.

eration to be paid in the form of the bidder's stock and the relative size of the deal increase. After controlling for endogeneity, we find that the average market reaction to merger announcements that are accompanied by concurrent conference calls is significantly more favorable than the average market reaction to announcements where the acquirers rely exclusively on press releases to introduce the deals. Based on a content analysis of a limited subsample of stock acquirers, we provide evidence that the greater volume of information provided in conference calls relative to press releases, as well as the greater emphasis on forward-looking details in conference calls relative to press releases, contributes to the more favorable returns to merger announcements that are accompanied by conference calls. We find no evidence that the superior announcement returns subsequently reverse or that conference calls are associated with pre-announcement abnormal accruals.

Overall, our study demonstrates that bidders use merger-related supplemental disclosure at the time of the merger announcement as a means to positively influence their stock prices. In addition, we document the usefulness of voluntary disclosure in the context of a significant strategic decision. More importantly, our analysis suggests that, in general, managers use conference calls around merger announcements as a means of conveying genuine private information as opposed to falsely hyping their stock prices.

APPENDIX

TAXONOMY USED TO CLASSIFY PRESS RELEASE AND CONFERENCE CALL DISCLOSURES

Historical Details

Deal Details
- Amount and form of consideration
- Signing of definitive agreement
- Determination and justification of purchase price
- Due diligence procedures/negotiations
- Additional details (collars, breakup fees, earnouts, etc.)
- Funding of merger
- Regulatory and other considerations
- Expected time to completion
- Approval status of target shareholders
- Financial advisor
- Merger premium
- Approval status of Board of Directors
- Historical relation between acquirer and target
- Other bidders
- Related fees and charges
- Approval status of acquirer shareholders

Acquirer Business
- Description of business
- Business model
- Description of products
- Description of acquirer assets
- Customer base
- Historical financial results
- Historical stock price performance
- Historical nonfinancial metrics

Target Business
- Description of business

Forward-Looking Details

Strategic Rationale
- Enumeration of strategic benefits/source of synergies
- Factors impacting strategy's success
- Merger as part of overall strategic vision
- Actions taken or to be taken to implement strategy
- General statements of optimism
- Customer overlap/new customer base
- Market expansion
- Product focus/distribution channel
- Position of combined entity in industry
- Quantification of cost savings
- Impact of merger on partners and customers
- Plans for future business combinations
- Impact of transaction on other strategic initiatives

Integration Details
- General discussion
- Technological integration
- Plans for facilities
- Personnel integration
- Cultural fit
- Fate of top management
- Discussion of the team responsible for integration
- Past successes in integration

Financial Guidance
- Guidance about income statement impact
- Accretive/decretive
- Hypothetical combined results
- Qualitative guidance

(continued on next page)

Historical Details

Target Business
- Description of target assets
- Description of products
- Target's historical financial results
- Business model
- Market position
- Management team/employees
- Customer base
- Revenue mix
- Strategic partnerships

Forward-Looking Details

Financial Guidance
- Effect of merger on nonfinancial measures
- Update of previously issued guidance
- Accounting for combination
- Hypothetical combined balance sheet
- Capital expenditure forecast
- Sarbanes-Oxley/SEC reviews
- Capital structure of combined entity
- Impact on key ratios/rate of return estimates
- Impact on specific segments

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The Role of Newswires in Screening and Disseminating Value-Relevant Information in Periodic SEC Reports

Edward Xuejun Li
University of Rochester

K. Ramesh
Rice University

Min Shen
George Mason University

ABSTRACT: We examine the role of newswires in identifying and conveying market-moving information in periodic SEC reports to capital market participants. Using data on Dow Jones Newswires, we find that newswires are more likely to send alerts on firms that do not release preliminary earnings, have credit ratings, are included in major market indices, have litigation exposure, or report losses. Reflective of the market's focus on certain key events, firms with a nonstandard audit opinion, in the process of delisting, reporting unusual accounting items, or raising equity capital also receive alerts. Moreover, not only do we find significant price and volume reactions to the alerts at the daily level, but also we document immediate intra-day market activity triggered by the alerts, whereas we detect no similar reaction for SEC filings that trigger the alerts. Additional analysis suggests that the intra-day reaction is not driven by noise trading.

Keywords: *information intermediaries; newswires; periodic SEC reports; information dissemination; market microstructure.*

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I. INTRODUCTION

This study examines the role of newswires as an information intermediary in capital markets. We focus on Dow Jones Corporate Filing Alert (hereafter, DJCFA), a service offered by Dow Jones Newswires that screens for and disseminates important market-moving information in SEC filings to professional investors and other market participants in a timely manner.[1] What firm and informational characteristics in a periodic report would motivate a newswire service to issue an alert? Given that the SEC filings are already publicly available on EDGAR, is the information disseminated by the newswire service incrementally value-relevant to investors? We address these two questions in this study.

Despite extensive research on the role of financial analysts in the capital market, there is only limited evidence on the economic function of *pure* information intermediaries such as newswires.[2] We choose a context in which information intermediaries must be highly skilled to identify market-moving information. As earnings press releases become increasingly preemptive (Francis et al. 2002; D'Souza et al. 2010a), swift identification of any value-relevant information in periodic reports requires astute information screening by intermediaries. In addition, despite the sophistication of U.S. capital markets, academic research suggests that the confluence of demand- and supply-side effects restricts the market's processing ability when it is inundated with information or when facing complex information (Hirshleifer et al. 2008; You and Zhang 2009), which is commonly the case in periodic reports (Griffin 2003; Li and Ramesh 2009a).

We posit that even large investors generally do not satisfy the cost-benefit calculus for combing through periodic reports of every stock they own or in their investment possibility set to identify market-moving information. The broad market demand for corporate accounting information, coupled with the prevalence of high fixed costs and close-to-zero marginal costs in the market for information goods (Romer 1990), provides the impetus for information intermediaries such as DJCFA to enter the marketplace. To quote Dow Jones:

> From initial public offerings to Chapter 7 liquidations, Dow Jones Corporate Filings Alert offers readers invaluable information and analysis beyond news available in press releases. As the volume of company filings to the SEC keeps growing, we can save our readers time and give them the edge they need in an information-overloaded environment. ("Dow Jones Newswires Re-Launches Corporate Filings Alert; Division Will Also Open First Bureau in Wilmington, Delaware," April 2, 2001, Business Wire)

Our analysis is based on a comprehensive sample of 26,615 alerts pertaining to 20,797 periodic SEC reports issued by Dow Jones over the post-EDGAR period 1997–2004, representing 9 percent of all periodic reports in our sample. While roughly 82 percent of the filings with alerts receive only one alert, another 12 percent receive two, with the remainder receiving three or more. The number of news categories in an alert ranges from one to seven, with a mean value of two.[3] While Dow Jones takes 2.3 weekdays on average to release an alert after the corresponding filing, 68 percent of the initial alerts are released within 24 hours.

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[1] The alerts we examine are mostly recitation of specific facts already disclosed by companies in their periodic SEC reports and, therefore, are unlikely to reflect the *quid pro quo* relation found between reporters and their corporate sources (Dyck and Zingales 2003). We understand that Dow Jones News Services has strict conflict-of-interest policies to ensure independence in coverage of companies. Also, given the factual nature of the Dow Jones alerts, they do not suffer from the spin effect presented in the typical news stories (Dyck and Zingales 2003; In et al. 2007).

[2] We view a pure information intermediary as "an independent, profit-maximizing information processing system performing its activities (information acquisition, processing, and distribution) on behalf of other agents' information needs" (Rose 1999, 79). Academic research on newswires as an information intermediary include Dawkins and Bamber (1998) who find that the adverse market reaction to corporate bankruptcy filings is mostly due to information in bankruptcy filings disseminated through Broad Tape, and Dopuch et al. (1986) who find that the stock market reacts adversely only to audit qualifications discussed by media. Also see Dyck and Zingales (2003), Bhattacharya et al. (2009), Fang and Peress (2009), Solomon (2009), Soltes (2009), and Bushee et al. (2010) on the broader role of business press in shaping the capital market information environment. See D'Souza et al. (2010b) on the role of capital market data aggregators.

Our first analysis examines the demand-and-supply considerations in the information market that guide the alert decision. Specifically, we posit that the market demand for information arises from investor awareness and the information environment. Consistent with the investor awareness argument (Wurgler and Zhuravskaya 2002; Chen et al. 2004; Chen et al. 2006; Docking and Dowen 2006; Elliott et al. 2006; Mase 2006; D'Souza et al. 2010b), firms included in S&P major market indices are more likely to receive a Dow Jones alert. Reflective of equity and credit market demands, analyst following and coverage by credit-rating agencies are positively associated with the incidence of receiving alerts. Investors in loss firms are alerted more often, given that traditional profitability measures alone are insufficient for valuation purposes (Hayn 1995; Collins et al. 1997; Chen et al. 2002; Ajinkya et al. 2005). Firms skipping preliminary earnings announcements also experience a higher incidence of alerts, given that their periodic reports are more apt to include information hitherto not disclosed to the marketplace. Consistent with the value of identifying market-moving information, we find more alerts on firms from industries with higher litigation risk.

Among the key firm-specific events, firms raising equity capital, approaching delisting, experiencing large price reactions at preliminary earnings announcements, reporting extraordinary or special items, or receiving a nonstandard audit opinion are more likely to prompt an alert by Dow Jones. In terms of economic significance, nonstandard audit reports increase the probability of an alert by 5.1 percentage points, followed by firms without a preliminary earnings announcement (3.7 percentage points), periodic reports of firms with a credit rating (3.0 percentage points), firms subject to delisting (1.9 percentage points), firms listed in major market indices (1.2 percentage points), and firms facing a higher litigation risk (1.1 percentage points). In terms of supply-side effects, we find that Dow Jones' propensity to issue an alert drops when the market is inundated with a large number of periodic reports. We also provide evidence that Dow Jones switches resources away from periodic reports on days when there is increased incidence of earnings releases in the marketplace. Conditional on issuing an alert, we find that the time lag between issuing the initial alert and the filing of the periodic report is roughly 40 (22) percent shorter in firm-quarters without a preliminary earnings announcement (for firms in industries with higher litigation risk).

Our second set of analyses focuses on the information content of filing alerts. After controlling for earnings announcements and filing of periodic reports, we find significant price and volume reactions on the days when Dow Jones issues an alert. However, the reactions are limited to the initial alert regardless of whether the initial alert is followed by another alert. Using the *Alert Day* observations, we also find that, while legal- and bankruptcy-related alerts consistently generate higher price and volume reactions compared to the other news categories, alerts with missing subject codes generate significantly lower market reactions, consistent with constraints on immediate trading either based on algorithms or that require human intervention.

One obvious concern with using daily data is the possible endogeneity that market participants are more focused on filings that trigger an alert for reasons other than the information contained in the alerts themselves. We address this endogeneity by comparing the price and volume reactions over a narrow intra-day period surrounding the release of filing alerts with those of a pseudo-event matched on the weekday and release time of the alert. Compared to the 60-minute pre-event period, the absolute stock returns (trading volume) for each of the first five minutes following the alert are between 27 and 52 percent (18 and 69 percent) higher, with no similar reactions following pseudo-events. In addition, our formal statistical tests show that the price and volume reactions to alerts during the first 16 minutes (including the event minute) are significantly higher than those during the comparable pre- and post-event windows as well as those during the corresponding pseudo-event window. However, when we base our analysis on the actual filing time of periodic reports, we find no significant price or volume reactions. Our results for the alerts and filings hold when we split the sample observations into 10-K filings versus 10-Qs. More importantly, despite the fact that there are abnormally low price and volume reactions generally surrounding the release of 10-Q reports (Li and Ramesh 2009a), 10-Q-based alerts generate immediate price and volume reactions comparable to those generated by10-K-based alerts. In addition, we find significant volume reactions from both large and small trades to alerts, with small trades exhibiting a more pronounced volume activity. Finally, we conduct a battery of tests to show that the intra-day price/volume reactions to alerts are not merely due to noise trading or market over-reaction.

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[3] Dow Jones categorizes the alerts based on more than 200 subject codes, which we collapse into ten broad news categories. See Section III for details.

Overall, our study makes several contributions to the extant literature. First, capital allocation and social welfare are clearly dependent on the efficiency of financial markets (Jennings and Barry 1983). Our study provides large-sample evidence regarding the key role played by newswires in reducing information overload faced by market participants and improving price efficiency. Second, while the traditional asset-pricing literature assumes instantaneous information diffusion and complete market reaction to publicly available information (Merton 1987), the behavioral finance literature allows for bounded rationality and slow diffusion of information (e.g., Hong and Stein 1999). However, there is no substantial body of literature that examines the cost-benefit trade-offs market participants face in shaping the capital market information infrastructure. Our study helps to fill this gap by showing that newswire services such as DJCFA act as delegated information intermediaries in the capital market. Finally, our study is an early attempt in using market microstructure data to study the role of information intermediaries. This approach has broader application for parsing the pricing and trading implications in numerous contexts with confounding events (Brown et al. 1992; Ecker et al. 2006; Li and Ramesh 2009a).

Section II discusses the role of newswires in shaping the capital market information infrastructure and describes the Dow Jones alert service. Section III describes our sample screening procedures and presents descriptive evidence on the filing alerts. The analyses of factors that influence Dow Jones to issue an alert and the market reaction to the alerts are detailed in Sections IV and V, respectively. The final section provides a conclusion.

II. NEWSWIRES AND THE MARKET FOR INFORMATION IN PERIODIC REPORTS

In this section, we first postulate the role of newswires generally in the market for information in periodic SEC reports, followed by a discussion of the Dow Jones Corporate Filing Alert service, the specific newswire service that we study.

Role of Newswires in the Market for Information in Periodic SEC Reports

Consistent with earnings releases increasingly preempting periodic SEC reports (Francis et al. 2002; D'Souza et al. 2010a), Li and Ramesh (2009a) report no significant price or volume reactions surrounding the filing of quarterly periodic reports and find evidence of market reaction to

10-K reports only under limited circumstances.[4] When periodic reports contain key value-relevant information, in some circumstances managers themselves have incentives to highlight it to mitigate litigation risk (Li and Ramesh 2009a; Li and Ramesh 2009b). However, when such disclosure incentives are absent, the speed with which the market assimilates new information from periodic SEC reports must be determined by the demand and supply forces at work in the market for corporate accounting information. Recent research suggests that when corporate information releases cluster in calendar time (Hirshleifer et al. 2008) or contain extensive textual information as in periodic SEC reports (You and Zhang 2009), the stock market does not process the information in a timely fashion.

However, while most investors acting alone would find it formidable to perform detailed quantitative and textual analysis on periodic reports of all stocks in their investment set, the nature of market for information goods suggests that information intermediaries such as newswires would enter the marketplace acting as information-gathering agents and serve a multitude of principals. Romer (1990) suggests that, although the market for information goods is characterized by large fixed costs, it faces trivial reproduction and dissemination costs, thereby virtually eliminating marginal cost. Given the broad market demand for corporate accounting information, sophisticated information intermediaries can obtain economies of scale by spreading the large fixed cost of information retrieval and processing among many customers (Veldkamp 2006). We argue that these market characteristics provide the impetus for newswire services with extensive expertise in the financial information market to act as delegated information intermediaries to large groups of market participants for collecting key information from periodic reports.

Description of Dow Jones Corporate Filing Alert Service

Newswire services facilitate both the corporate supply of accounting information and the subsequent retrieval by various market participants. While Business Wire and PR Newswire act as agents of public corporations in the information-dissemination process, the major wire services such as Dow Jones, Reuters, and Bloomberg cater to user needs through sophisticated real-time delivery of business news and news alerts. We choose to examine the DJCFA of Dow Jones, Inc. for three reasons. First, Dow Jones is a leading provider of business content and information services to the capital markets through newswires and other channels. Second, DJCFA specifically targets SEC filings, the focus of this study. Third, the alerts issued by this service are publicly available through Factiva. Below we provide a brief description of the history and the information services provided by DJCFA.[5]

In 1989, Dow Jones acquired Federal Filings Inc., a private company that acted as an information intermediary by culling through the voluminous SEC paper filings to extract and disseminate key information to various capital market participants including Wall Street. Dow Jones initially provided the alerts under Federal Filings Newswires, which was relaunched as DJCFA in 2001. The purpose of the alert service is to screen for key information disclosed in SEC filings that were not preceded by a press release, identify various value-relevant information buried in SEC filings, and to offer market-moving alerts. Given that selective disclosure is prohibited under Reg FD, DJCFA monitors SEC filings to quickly draw professional investors' attention to key corporate disclosures in the post-Reg FD period.

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[4] Beyer et al. (2009) find that market reactions to voluntary disclosures are in general greater than that for mandatory disclosures, which suggests that periodic reports may play more of a confirmatory role, adding credibility to prior voluntary disclosures such as preliminary earnings announcements or management forecasts.

[5] This sub-section is based on our discussions with Rick Stine of Dow Jones, various news releases relating to DJCFA obtained through Factiva, online search, and other publicly available news sources. We limit our discussions to SEC filings although DJCFA also reviewed bankruptcy court filings and information sources.

During the period we study, DJCFA was staffed by editors and reporters with industry-specific expertise to search for market-moving items of interest to professional investors and Wall Street. Specifically, the DJCFA staff determines data-collection strategies and dissemination speed based on perceived market following and news value.[6]

The DJCFA service targets traders on the floor of major exchanges, at the trading desks of various brokerage houses, mutual funds, and hedge funds, although individual investors can access a large portion of real-time news from Dow Jones Newswires through *The Wall Street Journal Online* (http://www.wsj.com). Along with DJCFA, Dow Jones news is distributed to over 100 vendors. Market participants who need real-time information obtain Dow Jones news through distribution platforms hosted by Thomson Reuters, Bloomberg, FactSet, etc. Those interested in archival research rely on services such as Factiva.[7]

III. SAMPLE AND DESCRIPTIVE EVIDENCE ON DOW JONES CORPORATE FILING ALERTS

In this section, we first describe our sample construction, and then provide descriptive statistics on Dow Jones Corporate Filing alerts.

Sample Screening Procedure and Distribution

Our sample screening procedure is detailed in Table 1, Panel A. We ran a keyword search in Factiva Academic and obtained 36,984 entries for the period of 1997–2004.[8] Of these, 3,220 are advertisements for the DJCFA service, 1,996 are news articles related to a group of firms rather than an individual firm's periodic report, and 438 are rumors. Given that we focus on the alerts that follow 10-K/10-Q filings, we exclude alerts pertaining to 8-Ks, notices of non-timely filing, amended filings, and periodic filings of foreign companies. We further drop alerts for small business companies because they face different mandatory disclosure standards. Finally, we delete a small set of observations with alerts unrelated to SEC filings (230), alerts related to SEC filings other than 10-Ks/10-Qs (67), or alerts without filing time stamps (54). The final sample consists of 26,615 alerts. Panel B indicates that these alerts are related to 20,797 periodic SEC filings (hereafter, alerted filings). While more than 80 percent of these filings generate only a single alert, a few filings generate as many as five or more alerts.

Untabulated results show that, while the proportion of 10-K/10-Q filings with an alert monotonically declined from 1997 to 2000, it rebounded beginning 2001, reflecting Dow Jones' increased emphasis on the alert service after Reg FD.[9] When examined by industry, the alert sample is representative of the overall Compustat population, except that there are disproportionately more (less) alerted filings in the retail (financial services) industry.

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[6] While the Dow Jones alert service that we study is largely a recitation of information in periodic SEC reports, the service does require its reporters' judgment to choose what specific information to include in the alert. However, unlike capital market muckrakers (Foster 1979), business press (Miller 2006), or Dow Jones' other alert/newswire services, DJCFA includes neither critical analyses nor reporters' assessment of the market implications of the information in the alert.

[7] Beginning July 2, 2009, Dow Jones has restructured the DJCFA service for various financial reasons given the current market conditions. The restructured service will utilize limited DJCFA reporters along with the entire corporate reporting staff at the New York desk to look for market-moving alerts to be sent either as part of DJCFA or broadly through Dow Jones Newswires.

[8] Based on discussions with Factiva, we used the free text string "10-K" or "10-Q" or "10K" or "10Q" or "10KSB" or "10QSB" or "10-KSB" or "10-QSB" with "Dow Jones Corporate Filing Alerts" as the source. We searched the period beginning in 1997, the first full calendar year after EDGAR became effective, and ending in 2004, the last year for which we are able to consistently identify alerts related to periodic reports through *Factiva Academic*.

[9] See "Dow Jones Newswires Re-Launches Corporate Filings Alert; Division Will Also Open First Bureau in Wilmington, Delaware," April 2, 2001, Business Wire.

TABLE 1

Sample of Dow Jones Corporate Filing Alerts

Panel A: Sample Screening of Dow Jones Corporate Filing Alerts

Dow Jones Corporate Periodic Filings Alerts, 1997–2004	36,984
Less: advertisements	(3,220)
news related to a group of firms[a]	(1,996)
rumors[b]	(438)
Initial Sample of Alerts	31,330
Less: alerts after 8-K filings[c]	(514)
alerts after filing of notice of delay under Rule 12b-25[d]	(1,881)
alerts after amended filings[e]	(233)
alerts for foreign firms who file only 20-F/6-Ks with the SEC	(613)
alerts from SB filings[f]	(1,123)
alerts related to non-filing news[g]	(230)
alerts related to non-periodic filings (S-4, 13D, DEF 14A, etc.)	(67)
alerts without filing time[h]	(54)
Final Sample of Alerts	26,615

Panel B: Frequency of Corporate Filing Alerts of Each Periodic SEC Filing

	Frequency	%	Total
1	17,051	81.99	17,051
2	2,529	12.16	5,058
3	750	3.61	2,250
4	269	1.29	1,076
5	126	0.61	630
Above 5	72	0.35	550
	20,797	100.00	26,615

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[a] We identify this type of news by searching the news title for keywords such as "corporate filing alerts: the morning's top news," "federal filings business news," "CFA early summary," "high yield information," "daily market wrap-up," etc.

[b] Keywords to identify this type of news are "notice of rumor" or "daily rumor."

[c] Alerts are included in this group if (1) the source of a corporate filing alert indicates 8-K filings; or (2) the firm filed an "8-K" or "8-K/A" on or one day before the alert day, and no 10-Ks/10-Qs were filed within 20 weekdays prior to the alert day.

[d] Alerts are included in this group if (1) the source of corporate filing alert is "NT 10-K" or "NT 10-Q"; (2) the news title contains keywords such as "delayed," "not timely," "extension to file," "files NT," etc.; or (3) the firm filed a form NT 10-K or NT 10-Q on or a day before the alert day, and no 10-Ks/10-Qs were filed within 20 weekdays prior to the alert day.

[e] Alerts are included in this group if (1) the source of corporate filing alert indicates amended filings; (2) the news title contains keywords such as "files amended"; or (3) the firm filed a "10-K/A," "10-K405/A," or "10-Q/A" on or one day before the alert day, and no 10-Ks/10-Qs were filed within 20 weekdays prior to the date of alert.

[f] Alerts are included in this group if (1) the source of corporate filing alert is "10KSB" or "10QSB"; or (2) the firm filed a form 10KSB or 10QSB form on or a day before the alert day.

[g] These alerts are related to earnings announcements, bankruptcies, Moody's downgrading, lawsuits, etc.

[h] This group consists of 26 alerts with corresponding 10-K/10-Q filings available from EDGAR but not in the SEC filing time database and 28 alerts having no corresponding filings from either the EDGAR or the SEC filing time database.

News Content of Corporate Filing Alerts

To analyze news content, we group the more than 200 subject codes provided by Dow Jones Newswires into ten broad news categories (see note in Table 2). For the 26,013 alerts with

TABLE 2

Descriptive Statistics on Alerts by News Categories

Panel A: Frequency Distribution of the Number of News Categories in Alerts

Number of News Categories	Frequency	%	Total
1	10,875	41.81	10,875
2	8,880	34.14	17,760
3	4,313	16.58	12,939
4	1,517	5.83	6,068
5	355	1.36	1,775
6	60	0.23	360
7	13	0.05	91
Total	26,013	100.00	49,868

Panel B: Frequency Distribution of News Categories

Performance (*PER*)	23,785	47.7%
Credit Related (*CRE*)	8,422	16.9%
Equity Related (*EQU*)	4,711	9.4%
Business Structure Change (*BSC*)	2,993	6.0%
Corporate Legal Issue (*LGL*)	2,776	5.6%
Employment Related (*EMP*)	2,635	5.3%
Corporate Business (*BUS*)	920	1.8%
Forecast (*FOR*)	753	1.5%
Bankruptcy (*BKC*)	528	1.1%
Tax Related (*TAX*)	296	0.6%
Other (*OTH*)	2,049	4.1%
Total	49,868	100.0%

We group the more than 200 news subject codes provided by Dow Jones into ten broad news categories, the details of which are available from the authors. Of the 26,615 filing alerts in Table 1, we exclude 326 alerts with missing subject codes and 276 alerts whose subject codes are too broad to be meaningful (e.g., "Corporate/Industrial News," "Commodity/Financial Market News"). Performance news (*PER*) includes information on sales, earnings, and dividends. Credit (*CRE*) (equity news (*EQU*)) provides information on debt (equity) issuances/changes. Business structure changes (*BSC*) capture major corporate events such as mergers and acquisitions, joint ventures, divestures and spin-offs, while corporate business news (*BUS*) provides details on government and nongovernment contracts, new products/services, intellectual property, licensing agreements, research and development, and outsourcing. Issues such as management turnover, executive compensation, layoffs, or insider stock transactions are classified as employment-related news (*EMP*). In addition, we have news groups corresponding to management forecasts (*FOR*), bankruptcy (*BKC*), legal issues (*LGL*), and tax matters (*TAX*). Subject codes that cannot be easily classified into any of the above news categories are included in "Other" (*OTH*).

non-missing subject codes, we identify 49,868 news categories. Table 2, Panel A shows that the number of news categories assigned to an alert ranges from one to seven, and the majority of alerts have more than one news category. Panel B shows that around 50 percent of categories are performance related, which is not surprising given that the primary objective of Dow Jones Corporate Filing Alerts is to highlight market-moving information. The second and third largest groups are credit- and equity-related news, followed by alerts relating to business structure changes, legal issues, and employment matters. Forecasts, bankruptcy, and tax matters are the smallest groups, accounting in total for only three percent of all news categories.

Untabulated analysis indicates that 10-K alerts are more about credit and equity arrangements, employment-related issues, and bankruptcy than 10-Q alerts, possibly due to business events related to these news categories occurring more frequently around the finalization of the annual report. In contrast, the higher incidence of performance-related news alerts based on 10-Q reports is consistent with Dow Jones grabbing the market's attention when it is likely to be less attentive (Barber and Odean 2008; Li and Ramesh 2009a). Another piece of untabulated descriptive evidence is that, while filings made within trading hours contain significantly more performance-related news, those made after trading hours contain more news in all other categories with several of the categories showing statistically significant differences. One conjecture is that firms try to avoid market attention to non-performance-related news by filing periodic reports after trading hours.

Timing of Dow Jones Corporate Filing Alerts

We obtain corporate filing alert release date/time from *Factiva Academic* and 10-K/10-Q filing date and time stamp from an EDGAR database. The alert release time stamp represents the time when an alert becomes available to the Dow Jones Corporate Filing Alert subscribers, and the 10-K/10-Q filing time stamp is when the periodic filing first appears in EDGAR.[10] Figure 1 plots the frequency of SEC filings and the corresponding initial alerts in blocks of 30-minute intervals. While the number of filings steadily accumulates over most of the SEC working hours of 8:30 a.m. to 5:30 p.m.,[11] the distribution of alerts is relatively smoother and saddle-shaped, with two small peaks at 11:30 a.m. and 3:30 p.m. Untabulated analysis indicates that the first hump for alerts in Figure 1 is caused by Dow Jones' heightened collection efforts to clear the backlog of periodic SEC reports filed during the previous business day.

We next examine the timeliness of corporate filing alerts by measuring the release lag as the number of weekdays between the periodic SEC report filing time and the release time of the first alert (*RL_WEEKDAY*).[12] Because 68 percent of the initial alerts are released within 24 hours after the SEC report is filed, we also measure the release lag in minutes (*RL_MINUTE*) for such alerts. Table 3, Panel A indicates that, on average, it takes Dow Jones Newswires 2.3 weekdays to release an alert after the corresponding SEC report is filed, with one weekday each for the median and the inter-quartile range. We also present statistics on the release lag for 10-K alerts and 10-Q alerts separately and find that the average *RL_WEEKDAY* is almost the same for the two groups. However, for alerts released within one weekday, the median collection time (*RL_MINUTE*) is 43 percent less for alerts based on 10-Q filings than 10-Ks (113 versus 197 minutes), consistent with increased complexity requiring additional time to review 10-K reports for market-moving information.

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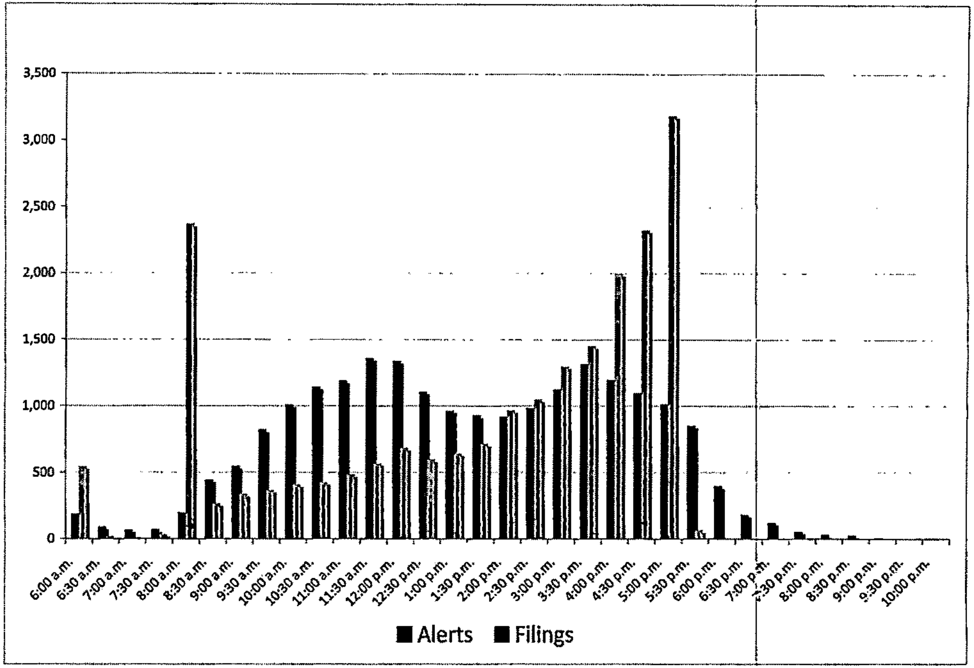
[10] Based on our discussions with SEC's Office of Information Technology, we understand that the periodic report time stamp is a good indication of when the filing could be accessed by users, but at least in the earlier years not everyone had instantaneous access to EDGAR (Balsam et al. 2002). When EDGAR was first rolled out, the Level 1 subscribers to EDGAR and their clients (which included many Wall Street firms and investment houses) had immediate access to the filings. The filings were not available at the public website for another 24 hours, which was changed in the year 2002 ("SEC Announces Free, Real-Time Public Access to EDGAR Database," http://www.sec.gov/news/press/2002-75.htm). Untabulated results show that (1) after year 2002 more alerts were issued within the first 15 minutes of filing; and (2) after eliminating these potentially confounding observations from our sample, alerts do (filings do not) trigger significant immediate market activity in periods both before and after the change in the public availability of periodic reports.

[11] Based on our discussions with SEC's Office of Information Technology, periodic reports filed before 5:31 p.m. Eastern time will receive a filing date and time identical to its EDGAR receipt date and time. Filings with a receipt time on or after 5:31 p.m. Eastern time will be assigned the next business day after the receipt date as the filing date and 6:00 a.m. as the filing time. Prior to 2003, the practice appears to have been the use of an 8:00 a.m. time stamp. The two spikes at 6:00 a.m. and 8:00 a.m. are consistent with the SEC time-stamping practice.

[12] We calculate *RL_WEEKDAY* as the number of hours elapsed from the time when a filing appears on EDGAR to the time when the first alert is released by DJCFA and divide it by 24. We subtract 2 if this timeframe contains a weekend. Similarly, the weekend is excluded in computing *RL_MINUTE*.

FIGURE 1
Calendar Time Frequency of SEC Filings and the Corresponding Initial Dow Jones Alerts



This figure presents the time distribution of 20,797 periodic SEC filings and the corresponding initial alerts between 1997 and 2004. The x-axis depicts the time in 30-minute intervals, and the y-axis is the number of periodic SEC filings and the corresponding initial alerts. Unreported analysis reveals similar patterns between 10-K and 10-Q filings/alerts.

Finally, we use the number of news categories in an alert to proxy for the news value to DJCFA subscribers and provide descriptive statistics on the release time of the first alert in Table 3, Panel B. We find that the mean of *RL_WEEKDAY* (the proportion of alerts released within 24 hours after filing) decreases (increases) almost monotonically with the number of news categories, consistent with Dow Jones staff placing higher priority on periodic reports with possibly multiple market-moving information.[13]

IV. DETERMINANTS OF NEWSWIRE ALERTS ON PERIODIC SEC REPORTS

In this section, we first develop predictions regarding the circumstances in which a newswire service is likely to issue an alert based on information in periodic SEC reports. Past research has not identified contexts in which information intermediaries more saliently disseminate to market participants information already disclosed by companies (Dopuch et al. 1986). Our predictions are

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[13] Our inferences from Table 3 are unaltered when we exclude observations relating to filings with a time stamp in the interval [6:00 a.m., 8:00 a.m.] Eastern time.

TABLE 3

Time Lag between Periodic SEC Filings and the Initial Corporate Filing Alerts

Panel A: Descriptive Statistics on Time Lag of 10-Ks versus 10-Qs

	RL_WEEKDAY			*RL_MINUTE*		
	Total	**10-K**	**10-Q**	**Total**	**10-K**	**10-Q**
n	20,797	6,929	13,868	14,069	4,632	9,437
Mean	2.325	2.325	2.324	453.187	513.396	423.634
Std.	3.269	3.350	3.227	516.042	520.629	511.215
Min	1	1	1	0	0	0
P5	1	1	1	8	12	7
Q1	1	1	1	40	61	34
Median	1	1	1	137	197	113
Q3	2	2	2	1,064	1,091	1,045
P95	8	7	8	1,337	1,344	1,331
Max	42	42	33	1,440	1,439	1,440

Panel B: Descriptive Statistics on Collection Speed by the Number of News Categories

Number of News Categories	Obs.	Mean of *RL_WEEKDAY*	Standard Deviation of *RL_WEEKDAY*	Proportion of One-Weekday Release
Missing	245	3.294	3.678	0.501
1	8,163	2.338	3.572	0.718
2	6,991	2.463	3.270	0.631
3	3,511	2.245	2.979	0.662
4	1,256	1.823	2.157	0.722
5	302	1.798	2.316	0.722
6	54	1.222	0.945	0.926
7	13	1.154	0.376	0.846

Panel A presents descriptive statistics on the release lag between the periodic SEC report filing time and the release time of the initial alert. *RL_WEEKDAY* is the release lag measured in weekdays, which is calculated as the number of hours elapsed from the time when a filing appears on EDGAR to the time when the first alert is released by DJCFA, divided by 24. Given that 68 percent of alerts are released within 24 hours after the corresponding periodic SEC reports are filed, we also calculate release lag in minutes, denoted *RL_MINUTE*, for these alerts. For both *RL_WEEKDAY* and *RL_MINUTE* we exclude weekends in the calculation. For 74 observations, their filing time stamp is post-alert, which is likely caused by data error. We replace the negative time lag with 0 for this analysis.

Panel B reports statistics on *RL_WEEKDAY* and the proportion of alerts released within 24 hours after filing for groups formed by the number of news categories in each alert.

based on firm characteristics that capture market demand for information and key firm-specific events likely to heighten market's attention on those firms. We then test our predictions using information on Dow Jones alerts.

Predictions

We posit that market demand for information relates to firm characteristics that capture investor awareness and information environment. Prior studies (Wurgler and Zhuravskaya 2002; Chen et al. 2004; Chen et al. 2006; Docking and Dowen 2006; Elliott et al. 2006; Mase 2006) suggest that firms whose shares are included in a major market index have increased investor awareness. Consistent with this argument, D'Souza et al. (2010b) find that data aggregators place higher collection priority on the financial statement information of these firms. Therefore, we predict that newswire services are more likely to target periodic reports of firms whose stocks are included in the S&P 500, S&P MidCap 400, or S&P SmallCap index (*SP1500*). All explanatory variables are defined in Table 4.

In addition, we expect that the nature of the information environment to be associated with the demand for newswire services. Extant research suggests that analyst following and institutional ownership are positively associated with the corporate information environment (O'Brien and Bhushan 1990; Frankel et al. 1999; Jiambalvo et al. 2002; Bushman et al. 2004; Piotroski and Roulstone 2004), but each may have a differential information search focus. As analysts have a competitive advantage in extracting industry-level information from firm performance (Piotroski and Roulstone 2004), we predict that newswire alerts could act as a complementary source by providing idiosyncratic information in firms with greater analyst following (*Analysts*). On the contrary, institutional investors are likely to expend greater resources in gathering firm-specific information (Jiambalvo et al. 2002; Piotroski and Roulstone 2004), which could result in newswire services competing with institutions' private search efforts. We therefore refrain from a directional prediction for institutional ownership (*Instown*). We consider the availability of a credit rating (*Rating*) as a proxy for creditor demand, thereby predicting a positive association with the existence of newswire alerts.

Academic research also suggests that when firms experience losses, market participants face an information environment in which traditional profitability measures may be lacking for valuation purposes (Hayn 1995; Collins et al. 1997; Chen et al. 2002; Ajinkya et al. 2005). As additional disclosures of loss firms may be relevant to the marketplace, we also predict that newswires would more often generate alerts for loss firms (*Loss*) to enable investors to better evaluate their financial circumstances.

Li and Ramesh (2009a) find that that a segment of firms first releases its quarterly earnings information only with the periodic reports (also see Amir and Livnat 2005). While the lack of a preliminary earnings release is consistent with lower value relevance of earnings and limited coverage by sophisticated market participants, periodic reports of these firms are more apt to include information hitherto not disclosed to the marketplace. Therefore, we expect newswire services to closely monitor the periodic reports of firms that did not provide a preliminary earnings release (*NoPrelim*). Finally, we consider exposure to litigation risk (*Litigation*) as another factor that is likely to influence newswire services to search for market-moving information in periodic reports.

We next identify key firm-specific events that create heightened market attention in general or point to the existence of potentially value-relevant information in periodic reports. With respect to general market attention, we expect that firms raising capital (*Stockissue* and *Debtissue*), that are takeover targets (*Takeover*), or confronting financial distress (*Ch11* and *Delisting*) face increased market scrutiny and, therefore, would be fitting targets of newswire services, as their periodic reports could contain useful information to interested stakeholders.

With respect to potentially value-relevant information, we first consider circumstances surrounding preliminary earnings releases that would enhance the confirmatory role of periodic reports (Beyer et al. 2009). Specifically, we argue that the market is more likely to demand confirming or negating information in periodic reports when preliminary earnings announcements generate large price reactions (*EA_mktr*) or when firms just meet or beat earnings targets (*JMOB*). In addition, we posit that firms reporting extraordinary items (*Extraordinary*) or special items (*Special*) and firms receiving a nonstandard audit opinion (*Nonstandard*) are likely to include in their periodic filings important information about the economic circumstances they face. Consequently, the periodic reports of such firms are likely to attract news alerts.

To test these predictions, we employ the following empirical model (firm and quarter subscripts are suppressed):

$$\begin{aligned} prob(Alert) = \Phi\Bigg(& \beta_0 + \beta_1 SP1500 + \beta_2 Analysts + \beta_3 Instown + \beta_4 Rating + \beta_5 Loss \\ & + \beta_6 NoPrelim + \beta_7 Litigation + \beta_8 Stockissue + \beta_9 Debtissue + \beta_{10} Takeover \\ & + \beta_{11} Ch11 + \beta_{12} Delisting + \beta_{13} ER\_mktr + \beta_{14} JMOB + \beta_{15} Extraordinary \\ & + \beta_{16} Special + \beta_{17} Nonstandard + \beta_{18} ROA + \beta_{19} Leverage + \beta_{20} Liquidity \\ & + \beta_{21} Arbrisk + \beta_{22} Volume + \beta_{23} nEA100 + \beta_{24} nFL100 + \sum_{T=1998}^{2004} \beta_{25,T} YT + \varepsilon \Bigg). \end{aligned} \tag{1}$$

As additional controls, we include three performance measures (*ROA*, *Leverage*, and *Liquidity*), given the documented relationship between firm performance and propensity for disclosures (Frankel et al. 1999). Moreover, we add controls for idiosyncratic volatility (*Arbrisk*) and trading volume (*Volume*), given that trading frictions can dampen the demand for corporate accounting information even among sophisticated investors (Collins et al. 2003; Ali et al. 2003; Mashruwala et al. 2006; D'Souza et al. 2010b). Finally, we include proxies for the overall market incidence of earnings announcements (*nEA100*) and periodic reports (*nFL100*) to capture supply-side constraints of newswire services when the market is inundated with corporate accounting information. D'Souza et al. (2010b) document similar effects for a data aggregator.

Empirical Analysis Based on Dow Jones Alerts

Our empirical analysis is based on a sample of 210,620 firm-quarters with a 10-K or 10-Q report, with Dow Jones issuing an alert in less than 9 percent of the cases (18,599/210,620). The low incidence is not surprising, given that the market focuses more on earnings announcements, which increasingly provide detailed financial statement information, and thereby preempt disclosures in periodic filings. Consequently, information intermediaries must have cost-effective screening mechanisms to identify value-relevant information buried in periodic reports.

Our untabulated univariate test results are largely consistent with our predictions. For instance, in support of the investor awareness argument, roughly 38 percent of the alert sample pertains to firms included in one of the S&P major market indices, compared to only about 20 percent in the non-alert sample. Similarly, the median analyst following (institutional ownership) in the alert sample is thrice (more than twice) that of the non-alert sample, consistent with equity holders' demand for information. The evidence on credit demand is strong, with half of the alert sample having an S&P credit rating, as compared to less than a quarter in the non-alert sample. Consistent with nontrivial supply-side effects, periodic reports that do not receive alerts are likely to be filed on days when the median number of filings made with the SEC is 542, as compared to 186 for periodic reports that received alerts.

Turning to the multiple regression analysis, model (1) we estimate using Chamberlain's Random Effects probit estimator, with the results presented in Table 4, Panel A.[14] In addition to reporting marginal effects (measured using partial average effects) and their p-values, we also report the economic significance of each independent variable as the marginal effect itself for the indicator variables and the marginal effect times the inter-quartile range for all other explanatory variables.

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[14] Two observations are in order. First, Chamberlain's Random Effects probit offers a consistent approach to incorporating unobserved firm-specific heterogeneity as well as the ability to estimate marginal effects. We do not consider fixed

The overall model is statistically significant, as indicated by the Wald Chi-squared statistic, with a Pseudo R^2 of 13.5 percent. Thirteen of the 16 predicted effects have statistically significant marginal effects with the correct signs at the two-tailed 0.10 level. Given that the unconditional probability of an alert from Dow Jones is around 0.09, the reported economic significance of many of the predicted effects appears material. We list below variables for which the economic significance is at least half of a percentage point and the statistical significance is at least at the 0.05 level:

Variable	***Nonstandard***	***NoPrelim***	***Rating***	***Delisting***	***SP1500***	***Litigation***	***Special***	***Loss***	***Stockissue***
Economic Significance	0.051	0.037	0.030	0.019	0.012	0.011	0.007	0.005	0.005

Taken together, we find that Dow Jones staff considers investor awareness, information environment, and key firm-specific events to identify potentially value-relevant information from periodic SEC reports. Of the control variables, the supply-side proxies show the largest effect on the incidence of alerts, with the probability of alerts declining by 0.017 when the intensity of earnings releases in the marketplace (*nEA100)* increases by its inter-quartile range (221). The evidence is consistent with the perceived priority of earnings announcements and, consequently, the diversion of Dow Jones' resources away from the periodic reports.

We next provide descriptive evidence on factors associated with the speed with which Dow Jones releases the alert.[15] In Table 4, Panel B we report the results of a pooled Poisson regression on the subsample with alerts in which the dependent variable is the alert lag (*RL_WEEKDAY*) and the independent variables are as in model (1).[16] Given that we use a Poisson regression, the economic significance levels we report are percentage changes in alert lag for a unit increment for all indicator variables and at inter-quartile range for all continuous variables (Wooldridge 2003, 574). Conditional on issuing an alert, we find that only a few of the variables are statistically significant in explaining the alert lag. However, we find some effects with large economic significance in that the alert lags are roughly 40 (22) percent shorter for firm-quarters without a preliminary earnings announcement (for firms with higher litigation risk). In addition, consistent with supply-side constraints, the alert lag is affected by information overload, with about a 9 (16) percent delay for an inter-quartile range increment in the number of earnings announcements (periodic SEC filings) released on the same day as the alerted filings.

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effects probit or fixed effects logit, as the former is inconsistent and the latter does not identify marginal effects (Wooldridge 2002, Chapter 15). Second, from an implementation standpoint, the Chamberlain's Random Effects approach models unobserved heterogeneity as a linear function of firm-specific means of all time-varying independent variables in the panel data plus an error term with zero mean and variance s^2_e. Consequently, the Chamberlain's Random Effects probit is a pooled regression that includes the firm-specific means of all time-varying independent variables as additional explanatory variables to control for unobserved heterogeneity at the firm level.

[15] Periodically, Dow Jones sends summary reports to its clients touting its ability to beat its competition in identifying market-moving information from SEC filings. For instance, in its March 2009 issue of Dow Jones Financial Services Solutions' *The Edge*, Dow Jones states that "on Feb. 16, the Friday ahead of a three-day weekend for the U.S., the Dow Jones Corporate Filings Alert scooped the competition with its report, unmatched by our competitors until four days later, that State Street Corp. disclosed billions of dollars in unrealized losses in its investment portfolio and the off-balance-sheet entities it manages."

[16] We do not report fixed effects Poisson results because the time-invariant indicator *Litigation*, which has significant influence on the alert lag, would be omitted from the regression. However, the inferences of fixed effects Poisson are very similar to those of pooled Poisson, except that *EA_mktr* and *Nonstandard* become insignificant.

TABLE 4

Determinants of the Issuance and Timing of Corporate Filing Alerts

Panel A: Determinants of Corporate Filing Alert

	Pred. Sign	Marginal Effect	p-value	Economic Sig.
SP1500	+	0.0116	(0.019)**	0.0116
Analysts	+	0.0006	(0.065)*	0.0031
Instown	+/−	−0.0004	(0.000)***	−0.0195
Rating	+	0.0299	(0.000)***	0.0299
Loss	+	0.0054	(0.017)**	0.0054
NoPrelim	+	0.0370	(0.000)***	0.0370
Litigation	+	0.0107	(0.000)***	0.0107
Stockissue	+	0.0053	(0.020)**	0.0053
Debtissue	+	−0.0013	(0.601)	−0.0013
Takeover	+	0.0082	(0.105)	0.0082
Ch11	+	−0.0141	(0.097)*	−0.0141
Delisting	+	0.0192	(0.001)***	0.0192
EA_mktr	+	0.0578	(0.002)***	0.0020
JMOB	+	−0.0005	(0.774)	−0.0005
Extraordinary	+	0.0067	(0.061)*	0.0067
Special	+	0.0071	(0.000)***	0.0071
Nonstandard	+	0.0512	(0.000)***	0.0512
ROA		−0.0582	(0.000)***	−0.0017
Leverage		0.0127	(0.078)*	0.0040
Liquidity		−0.0181	(0.051)*	−0.0078
Arbrisk		0.0012	(0.008)***	0.0035
Volume		0.0002	(0.000)***	0.0008
nEA100		−0.0078	(0.000)***	−0.0171
nFL100		−0.0007	(0.000)***	−0.0092
Year dummies?			Yes	
Obs.			147,119	
Pseudo R^2			0.135	
Wald Chi-squared			5,806.67	

Panel B: Determinants of Alert Lag (*RL_WEEKDAY*)

	Pred. Sign	Marginal Effect	p-value	Economic Sig.
SP1500	−	0.0092	(0.790)	0.0092
Analysts	−	−0.0035	(0.211)	−0.0177
Instown	−/+	−0.0003	(0.626)	−0.0128
Rating	−	0.0146	(0.662)	0.0146
Loss	−	−0.0233	(0.417)	−0.0233
NoPrelim	−	−0.3979	(0.000)***	−0.3979
Litigation	−	−0.2237	(0.000)***	−0.2237
Stockissue	−	−0.0023	(0.913)	−0.0023
Debtissue	−	0.0494	(0.242)	0.0494
Takeover	−	0.0202	(0.744)	0.0202
Ch11	−	−0.0316	(0.808)	−0.0316
Delisting	−	−0.0222	(0.749)	−0.0222

(continued on next page)

Panel B: Determinants of Alert Lag (*RL_WEEKDAY*)

	Pred. Sign	Marginal Effect	p-value	Economic Sig.
EA_mktr	–	−0.8969	(0.005)***	−0.0310
JMOB	–	−0.0248	(0.321)	−0.0248
Extraordinary	–	−0.0407	(0.314)	−0.0407
Special	–	−0.0106	(0.689)	−0.0106
Nonstandard	–	0.0969	(0.006)***	0.0969
ROA		0.0611	(0.691)	0.0018
Leverage		−0.0516	(0.366)	−0.0165
Liquidity		0.0625	(0.391)	0.0269
Arbrisk		−0.0071	(0.283)	−0.0208
Volume		−0.0017	(0.000)***	−0.0074
nEA100		0.0401	(0.000)***	0.0885
nFL100		0.0124	(0.000)***	0.1627
Year dummies?			Yes	
Obs.			14,940	
Pseudo R^2			0.096	
Wald Chi-squared			738.25	

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*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, and 0.01 levels, respectively.

Panel A reports the Chamberlain's Random Effects probit regression results of factors that trigger the issuance of Dow Jones corporate filing alerts, while Panel B reports the pooled Poisson regression results of factors that determine the alert lag between the periodic SEC report filing time and the release time of the initial alert, measured in weekdays. Given that the estimated Chamberlain's Random Effects coefficients are not directly interpretable, we report the marginal effects (calculated as the partial average effect) and their two-tailed p-values based on firm-clustered standard errors. The marginal effects in Panel B equal the estimated Poisson coefficients. The economic significance is the same as the marginal effect for indicators and calculated as the marginal effect multiplied by the inter-quartile range for continuous variables.

Variable Definitions:

SP1500 = 1 if the firm is in the S&P 1500 index at the end of current fiscal quarter, and 0 otherwise;
Analysts = number of analyst following over the current fiscal quarter; we obtain analyst following from the I/B/E/S database;
Instown = percentage of institutional stock ownership (winsorized at 100) at the end of the calendar quarter at or immediately preceding the current fiscal quarter; we obtain institutional ownership from Thomson Financial Spectrum;
Rating = 1 if S&P credit rating is available for the firm in the current fiscal quarter, and 0 otherwise;
Loss = 1 if net income before extraordinary items is negative for the current fiscal quarter, and 0 otherwise;
NoPrelim = 1 if the earnings information is first released through periodic SEC reports for the current fiscal quarter, and 0 otherwise. This includes cases where there is no preliminary earnings announcement, earnings announcement date concurs with the periodic SEC filing date, or the earnings announcement is made after the periodic SEC report is filed;
Litigation = 1 for industries found to be exposed to high litigation risk: biotech (SIC 2833–2936), computer hardware (SIC 3570–3577), electronics (SIC 3600–3674), retail (SIC 5200–5961), and computer software (SIC 7371–7379), and 0 otherwise;
Stockissue = 1 for any of the six quarters leading to the firm's stock issuance (including the quarter of stock issuance) and 0 otherwise; we obtain the stock issue information from *SDC*;
Debtissue = 1 for any of the six quarters leading to the firm's public debt issuance (including the fiscal quarter of debt issuance), and 0 otherwise; we obtain the public debt issue information from *SDC*;
Takeover = 1 for any of the six quarters leading to the firm's delisting from a stock exchange because of mergers and acquisitions (including the quarter of takeover), and 0 otherwise;
Ch11 = 1 for quarters [−5, 5] centered on the fiscal quarter when the firm files Chapter 11 petition, and 0 otherwise; we obtain the Chapter 11 bankruptcy information from Professor Lynn M. LoPucki's Bankruptcy Research Database;
Delisting = 1 for any of the six quarters leading to the firm's delisting from a stock exchange because of financial distress (including the quarter of delisting), and 0 otherwise;

(continued on next page)

EA_mktr = market reaction at the earnings announcement day, measured as absolute value of daily return minus value-weighted market return and winsorized at its top and bottom 0.5 percentile values; this variable is set to 0 if *NoPrelim* equals 1;
JMOB = 1 if the earnings surprise is between \$0.00 and \$0.03 per share, and 0 otherwise. Earnings surprise is measured as actual earnings per share (EPS) minus median of the latest five individual EPS forecasts made over a period from the previous quarter's earnings announcement to the current quarter's earnings announcement. Both actual value and forecasts of EPS are obtained from the unadjusted I/B/E/S database. If I/B/E/S data are not available for a firm-quarter, then we define earnings surprise as seasonally adjusted EPS (i.e., EPS in the current quarter minus EPS in the same quarter of prior year);
Extraordinary = 1 if an extraordinary item is reported in the Compustat database for the current fiscal quarter q, and 0 otherwise;
Special = 1 if a special item is reported in the Compustat database for the current fiscal quarter q, and 0 otherwise;
Nonstandard = 1 if the auditor's opinion on the firm's financial statements for the current fiscal year is not "unqualified," and 0 otherwise; we obtain auditor's opinion from the Compustat database (item #149) and this variable is set to 0 for interim quarters;
ROA = net income before extraordinary items divided by total assets at the beginning of the current fiscal quarter, winsorized at its top and bottom 0.5 percentile values;
Leverage = long-term debt divided by total assets at the end of the current fiscal quarter, winsorized at its top and bottom 0.5 percentile values;
Liquidity = sum of cash and cash equivalents and receivables divided by total assets at the end of the current fiscal quarter, winsorized at its top and bottom 0.5 percentile values;
Arbrisk = standard deviation of residuals from a regression of firm-specific daily returns on the returns of the CRSP equally weighted market index over the current fiscal quarter (a minimum of five observations is required); we multiply the standard deviation of residuals by 100 and winsorize it at its top and bottom 0.5 percentile values;
Volume = daily trading volume (in millions of dollars) averaged over the current fiscal quarter, winsorized at its top and bottom 0.5 percentile values;
nEA100 = number of all earnings announcements (in 100s) made on the day of the firm's filing of its periodic SEC report of the current fiscal quarter; and
nFL100 = number of periodic SEC filings (in 100s) released on the day of the firm's filing of its periodic SEC report of the current fiscal quarter.

V. DOES THE MARKET REACT TO NEWSWIRE ALERTS ON PERIODIC REPORTS?

In this section, we first use daily returns and trading volume to examine the market reaction to the alerts issued by Dow Jones. For those alerts issued during trading hours, we next examine intra-day price and volume reactions to isolate the immediate trading implications of the alerts and control for any endogeneity present in the analysis based on daily data.

Daily Returns and Volume Analysis

Using daily data over the period 1997–2004, we develop a multiple regression model to jointly examine market reactions to Dow Jones alerts after controlling for the effects of periodic filings and earnings announcements:

$$DV_t = \alpha + \sum_{Event=1}^{7} \sum_{Day=-2}^{2} \beta^{Event,Day} \cdot Indicator_t^{Event,Day}, \qquad (2)$$

where DV is a measure of daily stock price or volume reaction (defined below). The independent variables are 35 indicator variables corresponding to five event days $[-2$ to $+2]$ surrounding each of the following seven events:

- Dow Jones alerts;
- Two types of earnings announcements (interim, annual);

- Two types of 10-K reports (without an alert, followed by an alert); and
- Two types of 10-Q reports (without an alert, followed by an alert).[17]

We include separate indicator variables for periodic reports with and without a Dow Jones alert to control for any characteristics of the alert firm-quarters that might engender a differential reaction to the periodic filing.

Following Li and Ramesh (2009a), the dependent variable for stock returns is based on the absolute value of the difference between daily return and value-weighted market return. Following Cready and Mynatt (1991), the trading volume measure is based on the natural logarithm of the sum of volume and 0.000255, where volume equals daily trading volume divided by total outstanding shares. We standardize both the stock returns and trading volume measures by subtracting their firm-year mean and dividing by their firm-year standard deviation and denote them as *SAER* and *STV*, respectively.[18] Table 5, Panel A indicates that, after controlling for earnings announcements and periodic SEC filings, firm-quarters with a Dow Jones alert elicit statistically significant price and volume reactions on the event day (day 0).[19]

In Table 5, Panel B, we report the results of an expanded regression in which we break down the alert days into three sub-groups: (1) the initial alert days with a single alert; (2) the initial alert days containing multiple alerts; and (3) issuance of follow-up alerts subsequent to the initial alert day (coefficients of all other events are suppressed). The results indicate that the market reactions are limited to the initial alert day regardless of whether it is followed by another day with an alert. While the follow-up alerts may have market implications, our results based on daily data do not provide any evidence of their market-moving effects.

While this study and the extant research find no immediate price or volume reactions to the filing of 10-Q reports (Li and Ramesh 2009a; Lu 2006), one unanswered question is whether alerts based on 10-Q reports generate a market reaction, which we examine next. Given that the market reaction to follow-up alert days is not statistically significant, we focus on the effect of all initial alert days combined when comparing between 10-K and 10-Q alerts. The results in Table 4, Panel C show that both 10-K- and 10-Q-based alerts are associated with significant price and volume reactions (all other coefficients suppressed). Taken together, we find that initial alerts, whether based on annual or quarterly reports, provide incrementally value-relevant information.

We next provide evidence on the differential value relevance of various news categories. Given the general market reactions to all alerts, we estimate the following conditional regression based on 17,454 *Alert Day* observations with non-missing price and trading volume data:

$$\begin{aligned} DV_t = {} & \beta_0 + \beta_1 PER_t + \beta_2 CRE_t + \beta_3 EQU_t + \beta_4 BSC_t + \beta_5 LGL_t + \beta_6 EMP_t + \beta_7 CBS_t + \beta_8 FOR_t \\ & + \beta_9 BKC_t + \beta_{10} TAX_t + \beta_{11} OTH_t + \beta_{12} Missing_t + \beta_{13} Unclassified_t + \beta_{14} Multiple_t \\ & + \beta_{15} Second\ Alert\ Day_t + \beta_{16} Third\ and\ Following\ Alert\ Days_t + \varepsilon_t. \end{aligned} \tag{3}$$

Regression (3) includes indicators for each of the news categories as well as controls for alerts with subject codes missing (*Missing*) or not classifiable as firm-specific news (*Unclassified*). Given that the news categories are not mutually exclusive, we are able to include indicator variables for each of them. In addition, we include an indicator for days with multiple alerts

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[17] If the alerts or periodic reports are released after the market closes (i.e., post 4:00 p.m. Eastern time), we re-center the five-day event window around the next business day.

[18] In untabulated analysis, we use a Z-transformation of rank scores (Blom 1958) of absolute excess returns and share turnover as nonparametric measures for our dependent variables (Easton and Zmijewski 1993; Carter and Soo 1999; Li and Ramesh 2009a). The tenor of our market reaction results remains unaltered.

[19] See Li and Ramesh (2009a) for a discussion of a calendar quarter-end effect surrounding the filing of 10-K reports as well as evidence of a possible information transfer during quarter-ends. Our inferences regarding the market reaction to alerts are identical after controlling for the quarter-end effect (untabulated).

TABLE 5

Regression Results of Market Reaction Surrounding Corporate Filing Alerts

Panel A: OLS Regression Results of Market Reactions to All Alerts

		Standardized Absolute Excess Return (*SAER*)				
	Pred. Sign	**Day −2**	**Day −1**	**Day 0**	**Day 1**	**Day 2**
Event 1: Annual Earnings Release (n = 43,194)	+	0.027***	0.076***	0.433***	0.506***	0.125***
Event 2: Interim Earnings Release (n = 131,185)	+	0.043***	0.109***	0.473***	0.518***	0.127***
Event 3: 10-Ks without Alerts (n = 37,696)	+	−0.013	0.005	0.054***	0.046***	0.070***
Event 4: 10-Ks with Alerts (n = 5, 600)	+	−0.027	0.012	0.099***	0.055***	0.040***
Event 5: 10-Qs without Alerts (n = 120,668)	+	−0.063	−0.073	−0.057	−0.080	−0.054
Event 6: 10-Qs with Alerts (n = 10,659)	+	−0.060	−0.064	−0.016	−0.071	−0.056
Event 7: All Alerts (n = 17,561)	+	0.009	−0.023	0.033***	−0.011	−0.003

		Standardized Trading Volume (*STV*)				
	Pred. Sign	**Day −2**	**Day −1**	**Day 0**	**Day 1**	**Day 2**
Event 1: Annual Earnings Release (n = 43,194)	+	−0.020***	0.081***	0.503***	0.625***	0.341***
Event 2: Interim Earnings Release (n = 131,185)	+	−0.031***	0.064***	0.457***	0.564***	0.287***
Event 3: 10-Ks without Alerts (n = 37,696)	+	0.004	0.011**	0.049***	0.023***	0.034***
Event 4: 10-Ks with Alerts (n = 5,600)	+	−0.004	−0.005	0.037***	0.031**	0.047***
Event 5: 10-Qs without Alerts (n = 120,668)	+	−0.051	−0.066	−0.063	−0.083	−0.058

(continued on next page)

	Pred. Sign	Standardized Trading Volume (*STV*) Day −2	Day −1	Day 0	Day 1	Day 2
Event 6: 10-Qs with Alerts (n = 10,659)	+	−0.036	−0.035	−0.009	−0.060	−0.029
Event 7: All Alerts (n = 17,561)	+	−0.006	−0.032	0.029***	−0.014	−0.016

Panel B: OLS Regression Results of Market Reactions to Initial Alerts versus Follow-Up Alerts

	Pred. Sign	Standardized Absolute Excess Return (*SAER*) Day −2	Day −1	Day 0	Day 1	Day 2
Initial - Single Alerts (n = 14,453)	+	0.011	−0.029	0.033***	−0.016	−0.004
Initial - Multiple Alerts (n = 1,799)	+	0.012	0.039*	0.056**	0.023	0.007
Follow-up Alerts (n = 1,309)	+	−0.005	−0.023	0.014	0.006	−0.006

	Pred. Sign	Standardized Trading Volume (*STV*) Day −2	Day −1	Day 0	Day 1	Day 2
Initial - Single Alerts (n = 14,453)	+	0.006	−0.021	0.044***	0.002	−0.004
Initial - Multiple Alerts (n = 1,799)	+	−0.037	−0.010	0.057**	0.011	−0.004
Follow-up Alerts (n = 1,309)	+	−0.024	−0.070	−0.043	−0.081	−0.049

Panel C: OLS Regression Results of Market Reactions to 10-K Alerts versus 10-Q Alerts

	Pred. Sign	Standardized Absolute Excess Return (*SAER*) Day −2	Day −1	Day 0	Day 1	Day 2
Alerts following 10-Ks (n = 5,598)	+	0.015	−0.040	0.043***	0.016	0.022*
Alerts following 10-Qs (n = 10,654)	+	0.008	−0.012	0.032***	−0.026	−0.015

(continued on next page)

	Pred. Sign	Standardized Trading Volume (*STV*)				
		Day −2	Day −1	Day 0	Day 1	Day 2
Alerts following 10-Ks (n = 5,598)	+	−0.001	−0.031	0.046***	0.018	0.027**
Alerts following 10-Qs (n = 10,654)	+	0.002	−0.014	0.045***	−0.005	−0.021

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*, **, *** Represent statistical significance at one-tailed 0.1, 0.05, and 0.01 levels, respectively.

This table reports OLS regression results of market reactions based on a sample of 12,201,115 daily observations between 1997 and 2004. *SAER* is calculated as absolute value of daily return minus value-weighted market return, while *STV* is calculated as log (volume + 0.000255), where volume equals to daily trading volume divided by outstanding shares. We normalize both measures by subtracting firm/year mean and then dividing by firm/year standard deviation. In the Panel A regressions, we include 35 indicators for event days [−2, +2] surrounding the following events: (1) Dow Jones alerts; (2) two types of earnings announcements (interim and annual); (3) two types of 10-K reports (without an alert, followed by an alert); and (4) two types of 10-Q reports (without an alert, followed by an alert). If the alerts or periodic reports are released after the market closes (i.e., post 16:00 Eastern time), we re-center the five-day event window to the next business day. In the Panel B regressions, we break down alert event days into three groups: initial alert days with a single alert; initial alert days containing multiple alerts; and issuance of follow-up alerts subsequent to the initial alert day. In the Panel C regressions, we further break down all initial alert days into 10-K- and 10-Q-based alerts. For brevity, we omit the coefficients for all other indicators in Panel B and Panel C as the inferences remain the same. The tenor of our results is similar when we use time-clustered standard errors to adjust for cross-sectional dependence.

(*Multiple*), and indicators for the second trading day with another alert(s) based on the same periodic report (*Second Alert Day*), and alert days beyond the second day for the same periodic report (*Third and Following Alert Days*).

The statistically significant return and volume model intercepts in Table 6 reflect the average market reactions reported in Table 5 for the alert subsample. When discussing Table 6 results, we limit our discussions to indicator variables with consistent price and volume reactions. Compared with other news categories, the legal- and bankruptcy-related alerts generate both higher price reaction and trading volume, whereas the alerts with missing subject codes generate significantly lower market reactions. The results for bankruptcy-related alerts are consistent with the deterioration in the conventional public sources of information on distressed companies coupled with the demand from sophisticated market participants for trading in such companies (Gilson 1995).[20]

The lower price and volume reactions to alerts without a specific news category suggest that immediate trading based either on algorithms or requiring human intervention is constrained in these circumstances. As in the expanded regression results, we find that follow-up alerts issued one or more days after the initial alert generate much less market activity. The lower market reaction for delayed alerts is consistent with the limits to any single information intermediary's ability to consistently identify market-moving information hidden in periodic reports long after their filing. Overall, while the market responds most strongly to legal- and bankruptcy-related alerts, alerts without a specific news category and delayed alerts may not be value-relevant.

Finally, we compare the results in Tables 4 and 6 to examine whether factors that influence Dow Jones' alert decisions also result in significant market reaction when alerts contain news relating to these factors. Given that the analysis in Table 6 is based on the conditional sample of alerts, the reported coefficients merely capture the incremental market reaction. To measure the total market reaction, we report the sum of the coefficient corresponding to each of the news categories and the intercept (Coeff. + Const.) in Table 6.[21] In Table 4 we find that Dow Jones is more likely to issue alerts for firms with a credit rating or when they issue equity. Consistent with this evidence, we find that equity-related (*EQU*) and credit-related (*CRE*) news categories generate significant return and volume reactions.

Intra-Day Returns and Volume Analysis

One obvious concern with using daily data is the possible endogeneity that market participants are more focused on firm-quarters with an alert for reasons other than the information contained in the alerts. We partially control for this possibility in Table 5 by including separate indicator variables for periodic reports with and without a Dow Jones alert in addition to indicators for the alerts themselves. In this subsection we attempt to isolate the price and volume reactions to a narrow intra-day period surrounding the release of Dow Jones alerts, thereby potentially eliminating other confounding effects.

We start with the subsample of alerts issued within trading hours (15,403), and then limit our focus to those alerts issued between 10:30 a.m. and 3:00 p.m. (11,059) to allow for the 60-minute

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[20] Consistent with our result for the legal-related alerts, our discussions with a money manager indicate that some institutional investors rely on information intermediaries to track litigation-related disclosures or information of companies they follow.

[21] Coeff. + Const. captures the total market reaction to an initial single alert that contains news content corresponding to the specific category (i.e., setting *Multiple*, *Second Alert Day*, and *Third and Following Alert Days* to zero).

TABLE 6
Regression Results of Market Reaction by News Categories

	SAER		*STV*	
	Coeff.	**Coeff. + Const.**	**Coeff.**	**Coeff. + Const.**
Performance (*PER*)	−0.0405 (0.195)	0.1236 (0.000)***	0.0026 (0.926)	0.0909 (0.000)***
Credit Related (*CRE*)	−0.0449 (0.021)**	0.1193 (0.000)***	−0.0196 (0.277)	0.0687 (0.028)**
Equity Related (*EQU*)	−0.0471 (0.028)**	0.1170 (0.001)***	−0.0292 (0.152)	0.0591 (0.080)*
Business Structure Change (*BSC*)	−0.0714 (0.004)***	0.0928 (0.012)**	−0.0133 (0.589)	0.0750 (0.036)**
Corporate Legal Issue (*LGL*)	0.0558 (0.079)*	0.2199 (0.000)***	0.0849 (0.002)***	0.1732 (0.000)***
Employment Related (*EMP*)	0.0257 (0.598)	0.1898 (0.001)***	0.1063 (0.022)**	0.1946 (0.000)***
Corporate Business (*BUS*)	0.0143 (0.800)	0.1785 (0.003)***	0.0924 (0.077)*	0.1807 (0.002)***
Forecast (*FOR*)	−0.0531 (0.049)**	0.1111 (0.004)***	−0.0055 (0.830)	0.0827 (0.021)**
Bankruptcy (*BKC*)	0.1403 (0.056)*	0.3045 (0.000)***	0.2341 (0.001)***	0.3224 (0.000)***
Tax Related (*TAX*)	0.0117 (0.883)	0.1759 (0.043)**	0.0361 (0.629)	0.1244 (0.127)
Other (*OTH*)	0.0383 (0.294)	0.2024 (0.000)***	0.0848 (0.004)***	0.1730 (0.000)***
Missing	−0.3026 (0.000)***	−0.1384 (0.055)*	−0.2693 (0.001)***	−0.1810 (0.014)**
Unclassified	0.0693 (0.448)	0.2335 (0.006)***	0.0816 (0.265)	0.1699 (0.011)**
Multiple	0.0576 (0.039)**		0.0386 (0.141)	
Second Alert Day	−0.0821 (0.012)**		−0.0991 (0.002)***	
Third and Following Alert Days	−0.0861 (0.218)		−0.2143 (0.003)***	
Constant	0.1641 (0.000)***		0.0883 (0.004)***	
R^2	0.0034		0.0060	

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*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, and 0.01 levels, respectively.

This table reports market reaction results by news categories based on 17,343 Alert Days with market reaction data. *SAER* and *STV* are as defined in Table 5. We create indicators for each news category as well as controls for alerts with subject codes missing (*Missing*) or not classifiable as firm-specific news (*Unclassified*). We cannot categorize 276 alerts whose subject codes do not indicate any firm-specific news categories (e.g., "Corporate/Industrial News," "Commodity/Financial Market News"). *Multiple* equals 1 if multiple alerts for the same filing are released in one day, and 0 otherwise. *Second Alert Day* (*Third and Following Alert Days*) equals 1 if it is the second alert day (third and following alert days) after the initial alert day, and 0 otherwise. For each news category indicator in the regressions, we report both estimated coefficient and the sum of the coefficient and the intercept. While the former describes the differential value relevance of each news category, the latter captures the total market reaction to an initial single alert that contains news content corresponding to the specific news category (i.e., setting *Multiple*, *Second Alert Day*, and *Third and Following Alert Days* to 0). Two-tailed p-values are calculated based on firm-clustered standard errors and presented in parentheses.

pre- and post-event windows that we examine.[22] After merging with the TAQ database, we have 7,913 alerts with complete trading data. In the spirit of Brown et al. (1992, 77–78), corresponding to each alert we identify a pseudo-event (i.e., a pseudo alert) as occurring at the same hour and minute as the alert and on the same weekday of the week that immediately follows the alert. We choose the pseudo-events after the issuance of the alerts so that earnings announcements are less likely to confound our inferences.

For both pseudo and actual alerts, our intra-day return and volume metrics are measured using the TAQ trade files by keeping only trades that meet all of the following criteria (Ng et al. 2008): (1) trades occurred on the NYSE, AMEX, or NASDAQ; (2) trades were made under regular market conditions; (3) trades were made within the normal trading hours (i.e., 9:30 a.m.–4:00 p.m.); (4) trades were good trades without subsequent cancellations; and (5) the transaction price and the number of shares traded were both positive. We consider a window starting 60 minutes prior to the event minute to 60 minutes following (a total of 121 minutes). For each minute in the event window, we calculate the stock return measure (*ABSRET*) as the absolute value of $\frac{(PRICE_t - PRICE_{t-1})}{PRICE_{t-1}}$, where $PRICE_t$ is the trading price of the last transaction within minute t and $PRICE_{t-1}$ is the trading price of the last transaction before minute t. If no transaction occurs within minute t, then *ABSRET* is set to 0. We use the number of shares traded within the minute deflated by shares outstanding as the metric for trading volume, labeled *SVOL*. Untabulated analysis using the un-deflated number of shares traded yields similar results.

In the two panels of Figure 2 we plot the average minute-by-minute *ABSRET* and *SVOL*, respectively, for the Dow Jones alerts versus the pseudo-events. In both cases, we find no noticeable market activity surrounding the pseudo-events, consistent with their proxy for non-event behavior. However, we find almost instantaneous price and volume reactions to the issuance of Dow Jones alerts, with persistent market movements for several minutes following the alert. Compared to the average *ABSRET* of the 60-minute pre-event period, the absolute returns for the first five minutes following the alert (including the minute of the alert) are between 28 to 52 percent higher, with the corresponding figures of −11 to 3 percent for the pseudo-events (untabulated).[23] Results based on *SVOL* are similar, with 18 to 69 percent higher trading activity following the alert, compared to between −4 and 4 percent following the pseudo-events (untabulated).[24] In comparison to the market reaction to the alerts, the untabulated analysis provides no evidence of incremental market movements following the filing of periodic reports that triggered one or more alerts.[25]

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[22] Two observations are in order. First, limiting our alerts to those issued between 10:30 a.m. and 3:00 p.m. could introduce another selection bias. However, we believe the benefits of our sample restriction outweigh any costs as the opening-of-day and end-of-day market effects documented in the finance literature (Gerety and Mulherin 1992) would inextricably confound our analysis based on the full sample. Moreover, we believe that the incentives of the newswire service should be unaltered by the timing of the alert, so the results based on our restricted sample should have broader implications. Second, Brown et al. (1992) use a similar restriction in their analysis to control for the opening-of-day and end-of-day effects.

[23] Kim et al. (1997) do not find price reactions to the public release of analyst recommendations by Dow Jones through the Broad Tape. The lack of a price reaction is not surprising given that Dow Jones is merely re-releasing information that was privately distributed to important analyst clients before the market opened. In fact, Kim et al. (1997) find that the information released privately to the clients was impounded during the first 5 to 15 minutes after the market opened. In our context, while the periodic reports are publicly available, Dow Jones screens for and identifies market-moving information from them. Indeed, our results suggest that the screening service of Dow Jones has informational value to the marketplace.

[24] We also find that the average number of trades in the first 16 minutes [0,+15] subsequent to an alert is 30 as compared to 24 in the same window following a pseudo alert, representing a 25 percent increase.

[25] In addition, we plot the price and volume activities of the 48,565 SEC filings during the sample period that did not receive a Dow Jones filing alert and are filed between 10:30 a.m. and 3:00 p.m. To address the quarter-end effect documented in Li and Ramesh (2009a), we divide the filings into three groups: 10-K QEND (3,594), 10-K NQEND

We next formally test the significance of the price and volume reactions to the release of Dow Jones alerts. We define the event window as consisting of the event minute corresponding to the alert (or SEC filing) time stamp and the 15 minutes immediately following the time stamp. To avoid confounds, we further limit our analysis to periodic reports and alerts that are more than 15 minutes apart. We define pre- and post-event periods of the same length (denoted [−16, −1] and [+16, +31]) for benchmarking. In addition, using the pseudo-events discussed above, we similarly define three event windows. For each of the 16-minute windows, we calculate the absolute return and share turnover measures. The significance of t-tests for the mean absolute returns and share turnover at the alert and SEC filing event windows are reported in Table 7, Panels A and B, respectively. We also report test results separately for quarterly and annual reports and their corresponding alerts.

In columns (4) and (5) of both panels, we compare the "true" event-window price or volume measure with that of the pre- and post-event window, respectively. In columns (9) and (10), we report similar test results, but based on pseudo-event windows. Finally, in column (11), we compare the "true" event-window price or volume measure with that of the pseudo-event-window. Focusing on the pseudo-event results for the alerts and the SEC filings in columns (9) and (10), the event-window returns and volume behave in a similar fashion to those in both pre- and post-event window, with only 4 out of 24 comparisons providing significant differences. Overall, the pseudo-event window market reaction appears to be a reasonable benchmark for testing the information content of Dow Jones alerts. In addition, the results are unlikely driven by a small number of trades, in that during the first 16 minutes subsequent to an alert (pseudo alert), 61.5 (59.2) percent of the observations indicate a price change and 75.0 (73.3) percent have nonzero trading volume (untabulated).

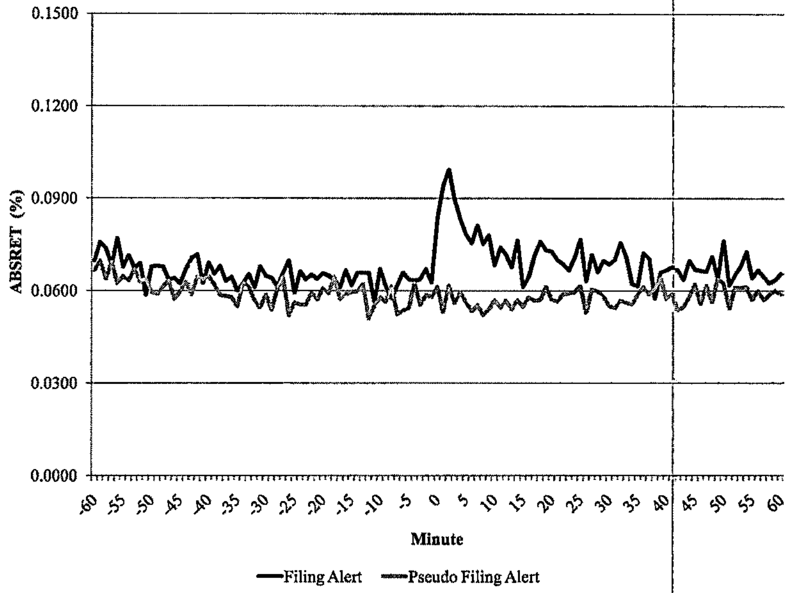
As shown in Table 7, Panel A for all alerts ("All"), when compared to the pre- or post-event window, we find a statistically significant increase in market activity at the event window capturing the release of Dow Jones alerts (columns (4) and (5)). This result holds when we benchmark the alert event window against the pseudo-event window (column (11)). Note that the economic significance is larger when we benchmark against the pseudo-event window. For instance, the alert event price reaction is 35 percent larger when compared to the pseudo-event, but it is only 18 percent larger when compared to the pre-event period. The corresponding figures for the trading volume are 47 percent for the pseudo-event comparison and 22 percent for the pre-event comparison.[26] When we break down alerts between those that are 10-K-based versus 10-Q-based, the results are largely consistent, in that both sub-groups show significant market reactions. The results in Panel B indicate that, when compared to the pre- or post-event period, the periodic SEC reports do not generate significantly larger price or volume activity during the 16-minute filing window using any of the three benchmarks.

(6,038), and 10-Q (38,933), where 10-K QEND (NQEND) refers to 10-Ks filed within (outside of) the [−1, +1] trading day window around any calendar quarter-end. 10-Q refers to all 10-Q filings. The price and volume charts for all three groups show no evidence of a market reaction around the filing of the periodic reports. This evidence dispels the concern that the lack of an immediate intra-day market reaction to filings that are followed by alerts may not be generalized to other periodic SEC filings. Consistent with Li and Ramesh (2009a), 10-Q graphs show no noticeable market activity throughout the event window, whereas non-quarter-end 10-K graphs show higher market volatility but similar price and trading levels when compared to the 10-Qs. The quarter-end 10-Ks are associated with the highest volatility and trading/price levels among the three groups, but the high volatility and trading levels are present during the entire 121-minute window, consistent with a quarter-end effect (Li and Ramesh 2009a).

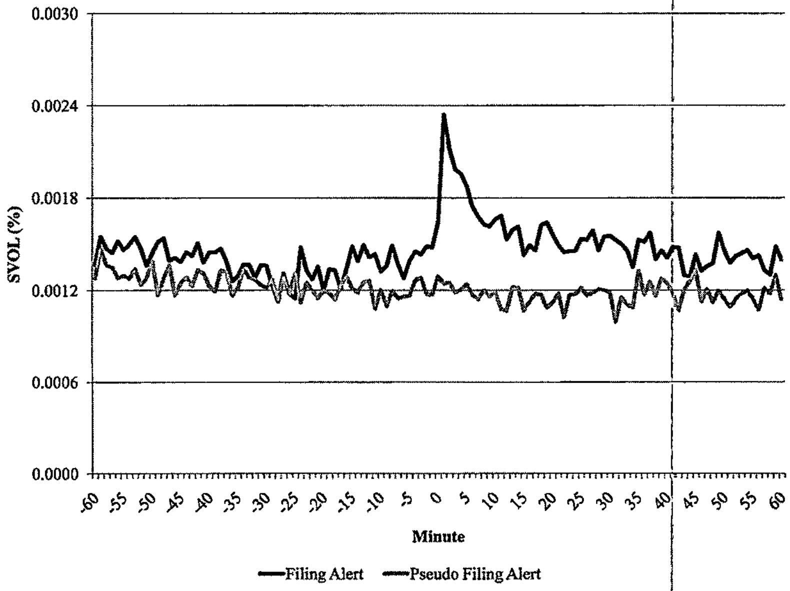
[26] Consistent with our analysis of determinants of alert lag (Table 4, Panel B), we find that the longer it takes for Dow Jones to issue an alert, the lower is the market reaction. One-standard-deviation increment in the number of minutes between the filing time and the alert time reduces the 16-minute stock return (volume) reaction by 15.8 (16.9) percent (untabulated).

FIGURE 2
Average Intra-Day Market Reaction to Dow Jones Filing Alerts, by Minute

Panel A: Absolute Return



Panel B: Shares Turnover



These figures are based on 7,913 Dow Jones filing alerts issued between 10:30 a.m. and 3:00 p.m. of trading days over 1997–2004. The corresponding pseudo-event window starts with the same hour and minute as the filing alert and on the same weekday of the week that immediately follows the filing alert. *ABSRET* is calculated as the absolute value of $\frac{(PRICE_t - PRICE_{t-1})}{PRICE_{t-1}}$, where $PRICE_t$ is the trading price of the last transaction within minute t and $PRICE_{t-1}$ is the trading price of the immediately previous transaction before minute t. If no transaction occurs within minute *t*, then *ABSRET* is set equal to 0. Shares turnover is the number of shares traded within the minute deflated by total shares outstanding (*SVOL*). All measures have been winsorized at their respective top 0.1 percent.

Sensitivity Analyses and Robustness Checks

To corroborate our findings, we conduct a series of untabulated sensitivity analyses and robustness checks. One concern about the immediate market reactions to Dow Jones alerts is that they may merely reflect noise trading as opposed to trading behavior consistent with improved price efficiency. If so, one would expect complete price reversals as informed traders enter to correct any mispricing or over-reaction. When we classify Dow Jones alerts into good and bad news based on the sign of the price reaction during the five minute window [0, +4], we find no evidence of any price reversal in the bad news group during the next 120 minutes. However, while we find a 30 percent price reversal in the good news group by the 30-minute point, there is no further reversal during the next 90 minutes. In addition, we find no inter-day price reversals when we regress *Alert Day* returns against returns of the next two days.[27] Overall, while there is some evidence of intra-day reversal in good news alerts, our primary results do not appear to be driven by noise trading.

To gain further insight into the intra-day market responses to Dow Jones alerts and periodic filings, we examine volume reactions separately for small versus large trades during the window [0, +15]. Following Bhattacharya (2001), we define a large (small) trade as any transaction with a dollar value higher than $50,000 (lower than $5,000). However, when the stock price is over $50, any transaction with less than or equal to 100 shares is also defined as a small trade. We calculate the standardized trading volume (*SVOL*) separately for large and small trades by deflating the corresponding raw volumes by total shares outstanding. Untabulated results show that: (1) both large and small trades exhibit significant volume reactions to the issuance of Dow Jones alerts in the first 16-minute event window; (2) small trade volume reaction in the event window exceeds that in the corresponding pseudo-event window by 83.4 percent, but the reaction is only 35.6 percent higher for large trades; and (3) periodic SEC filings generate insignificant volume reactions from both large and small trades. Overall, the large and small trade results echo the aggregate trading volume results. Finally, we find that large trades neither lead nor lag small trades, further refuting the alternative explanation of noise trading.[28]

Prior research suggests that bad news would attract more coverage by media and other information intermediaries (Gaa 2009), as managers have lower incentives to disseminate such information voluntarily (Kothari et al. 2009). Based on the first five-minute price response to the alert, we assign firm-quarters into bad news, good news, and no response categories. Using an ordered probit model, we find that none of the news categories in Table 6 is significantly associated with the sign of the market response. Taken together, our analysis provides a consistent picture of the market reacting immediately to the release of Dow Jones alerts. While we find no pervasive evidence of an immediate price or volume reaction to periodic reports, our results do not suggest that periodic reports never engender instantaneous market reaction. Future research could

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[27] In our inter-day analysis, we use a pseudo-event day (i.e., the same weekday in the next week) as the benchmark to control for the short-term price reversal documented in the finance literature (see discussions in Thomas and Zhang [2008]).

[28] While the total trading volume is not correlated with the level of institutional ownership, we find a statistically significant positive (negative) correlation between event-window volume due to large (small) trades and institutional ownership. To the extent that small trades are more representative of individual trading, DJCFA service is acting as an attention-grabber that facilitates trades by individual investors (Barber and Odean 2008).

TABLE 7

Intra-Day Absolute Returns and Trading Volume Surrounding Dow Jones Filing Alerts, SEC Filings, and Control (Pseudo-Event) Periods

Panel A: Dow Jones Filing Alerts

		Event				
		Event Window [0,+15]	Pre-Event Window [−16,−1]	Post-Event Window [+16,+31]	Event *Minus* Pre-Event	Event *Minus* Post-Event
		(1)	(2)	(3)	(4)	(5)
All	|Return|	0.485	0.409	0.440	0.075***	0.044***
(7,615)	Turnover	0.029	0.024	0.025	0.005***	0.004***
Alerts on 10-K	|Return|	0.532	0.397	0.491	0.134***	0.040
(2,704)	Turnover	0.027	0.021	0.024	0.006***	0.003**
Alerts on 10-Q	|Return|	0.459	0.416	0.412	0.043**	0.047***
(4,911)	Turnover	0.030	0.025	0.026	0.005***	0.004***

		Pseudo-Event (One Week after Event)					
		Event Window [0,+15]	Pre-Event Window [−16,−1]	Post-Event Window [+16,+31]	Event *Minus* Pre-Event	Event *Minus* Post-Event	Event *Minus* Pseudo-Event
		(6)	(7)	(8)	(9)	(10)	(11)
All	|Return|	0.357	0.380	0.369	−0.023*	−0.012	0.128***
(7,615)	Turnover	0.020	0.020	0.019	0.000	0.001	0.009***
Alerts on 10-K	|Return|	0.385	0.402	0.397	−0.017	−0.012	0.147***
(2,704)	Turnover	0.022	0.021	0.020	0.000	0.001	0.005**
Alerts on 10-Q	|Return|	0.342	0.368	0.354	−0.026*	−0.012	0.117***
(4,911)	Turnover	0.019	0.019	0.018	−0.001	0.000	0.011***

(continued on next page)

Panel B: SEC Filings Tagged by Alerts

		Event				
		Event Window [0,+15]	Pre-Event Window [−16,−1]	Post-Event Window [+16,+31]	Event *Minus* Pre-Event	Event *Minus* Post-Event
		(1)	(2)	(3)	(4)	(5)
All	|Return|	0.332	0.333	0.358	0.000	−0.026
(3,669)	Turnover	0.019	0.018	0.020	0.000	−0.002**
10-K Filings	|Return|	0.318	0.337	0.346	−0.019	−0.028
(1,088)	Turnover	0.019	0.018	0.020	0.001	−0.001
10-Q Filings	|Return|	0.339	0.331	0.363	0.008	−0.025
(2,581)	Turnover	0.018	0.018	0.020	0.000	−0.002**

		Pseudo-Event (One Week after Event)					
		Event Window [0,+15]	Pre-Event Window [−16,−1]	Post-Event Window [+16,+31]	Event *Minus* Pre-Event	Event *Minus* Post-Event	Event *Minus* Pseudo-Event
		(6)	(7)	(8)	(9)	(10)	(11)
All	|Return|	0.333	0.326	0.353	0.007	−0.020	−0.001
(3,669)	Turnover	0.018	0.018	0.019	0.000	−0.002***	0.001
10-K Filings	|Return|	0.358	0.334	0.378	0.024	−0.020	−0.040
(1,088)	Turnover	0.017	0.016	0.019	0.001	−0.001	0.002
10-Q Filings	|Return|	0.323	0.323	0.343	0.000	−0.020	0.015
(2,581)	Turnover	0.018	0.018	0.020	−0.001	−0.002**	0.001

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*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, and 0.01 levels, respectively.

This table is based on the sample period 1997–2004.

Panel A presents the absolute returns and shares turnover during the 16-minute alert window and the corresponding pseudo-event window. All alerts and the two subsamples—10-K-based alerts and 10-Q-based alerts—are shown separately with the respective numbers of observations in parentheses. Only Dow Jones filing alerts issued (1) between 10:30 a.m. and 3:00 p.m. of trading days and (2) beyond 15 minutes after the corresponding SEC filings are included in this analysis. The corresponding pseudo-event window starts with the same hour and minute as the filing alert on the same weekday of the week that immediately follows the filing alert. |Return| is the absolute value of return. Turnover is number of shares traded divided by total shares outstanding. Both metrics have been winsorized at top 0.1 percent and are shown in percentage (%). Event Window refers to the 16 minutes [0, +15] relative to the filing alert. Pre-Event (Post-Event) Window refers to the 16-minutes period immediately before (after) Event Window. The significance tests on difference are based on regressions with the difference as dependent variable regressed on the intercept, after controlling for heteroscedasticity and firm-specific clustering. Panel B presents similar metrics for SEC filings tagged by alerts. Only SEC filings that (1) are filed between 10:30 a.m. and 3:00 p.m. of trading days and (2) are tagged by at least one alert with the first alert issued beyond 15 minutes after the SEC filings are included.

identify circumstances that contribute to immediate processing and reaction to information in periodic reports, which could help regulators grapple with the information mosaic of the mandatory disclosure system currently in place.

VI. CONCLUSION

As capital markets continue the transition into the information age, market participants face not only exponential growth in the quantum of available financial information, but also rapid increases in the number of channels for obtaining information. The new information landscape can stifle even sophisticated investors as they encounter information overload. Consequently, major capital market participants are increasingly relying on internal resources as well as delegated external information intermediaries to screen the influx of corporate financial reports and identify value-relevant information on a timely basis to make informed investment decisions.

Our study focuses on the Corporate Filing Alert service provided by Dow Jones, Inc., which seeks to identify and convey important market-moving information buried in lengthy periodic SEC reports to market participants. We find that for all 10-K/10-Q reports filed between 1997 and 2004, only 9 percent received one or more Dow Jones alerts. Sixty-eight percent of the alerts are issued within 24 hours after the release of corresponding SEC reports.

In general, we posit that Dow Jones' decision to issue an alert is driven by newsworthiness and, therefore, by the perceived value that investors might place on the selected information in periodic SEC reports. We consider investor demand arising from investor awareness and information environment. We find evidence that the likelihood of receiving alerts increases in firms that do not release preliminary earnings, have credit ratings, are included in major market indices, have litigation exposure, or report losses. Among key firm-specific events, firms with a nonstandard audit opinion, in the process of delisting, reporting unusual accounting items, or raising equity capital are more likely to receive an alert. Conditional on issuing an alert, alerts are issued faster on the periodic reports of firm-quarters without a preliminary earnings release or firms from industries with high litigation risk.

Regarding the information content of alerts, we document significant price and volume reactions on the day when news alerts are issued, with bankruptcy- and legal-related news generating the largest reactions. To circumvent any selection bias in firm-quarters followed by DJCFA, we examine the 121-minute event window surrounding the release of alerts, finding that both absolute stock returns and turnover are significantly higher in the 16-minute window immediately following the alert compared with the pre-event or pseudo-event period. The market reactions to alerts are not due to noise trading, although both large and small trades contribute to the reactions. Taken together, our results support the conclusion that Dow Jones alerts convey value-relevant information to investors. Future research could examine whether the documented market reaction reflects both real and informational effects of Dow Jones alerts.

From a regulatory standpoint, the current SEC Commissioner Troy A. Paredes noted that "[t]he federal securities laws primarily focus on … mandating disclosure. Relatively little attention is paid to how the information is used—namely, how investors and securities market professionals search and process information and make decisions based on the information the federal securities laws make available" (Paredes 2003, 418).[29] Our study suggests that the SEC should consider expanding both the breadth and depth of the interactive disclosure requirements. To the extent that mandatory disclosures become more technologically friendly, capital market information intermediaries are bound to leverage the technology by designing faster and better-targeted information search and dissemination strategies to further enhance the efficiency of U.S. capital markets.

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[29] Commissioner Troy A. Paredes' writing that we cite was published when he was an academic.

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Stock Repurchases and Executive Compensation Contract Design: The Role of Earnings per Share Performance Conditions

Steven Young
Lancaster University Management School
Jing Yang
Towers Perrin

ABSTRACT: We examine the link between firms' stock repurchase activity and the presence of earnings per share (EPS) performance conditions in executive compensation contracts. Findings reveal a strong positive association between repurchases and EPS-contingent compensation arrangements. Further analysis suggests net benefits to shareholders from this association. Specifically, repurchasers experience larger increases in total payouts; the positive association between repurchases and cash performance is more pronounced for firms with EPS targets in the presence of surplus cash; undervalued firms with EPS targets are more likely to signal mispricing through a repurchase; and repurchasers with EPS conditions are associated with lower abnormal accruals. We find no evidence that EPS-driven repurchases impose costs on shareholders in the form of investment myopia.

Keywords: *corporate payout policy; performance targets; earnings management; efficient contracting.*

Data Availability: *The data are available from public sources identified in the study.*

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I. INTRODUCTION

This study investigates the link between firms' stock repurchase activity and the presence of earnings per share performance conditions in executive compensation contracts. Our analysis seeks to address the apparent disconnect between theory and practice regarding repurchases. One the one hand, traditional academic theories identify factors such as signaling (Vermaelen 1981), agency costs (Fenn and Liang 2001), and leverage (Dittmar 2000) as important determinants of repurchase activity. On the other hand, survey and anecdotal evidence highlight earnings per share (EPS) as a key factor influencing managers' repurchase decisions (Badrinath and Varaiya 2000; Brav et al. 2005; Caster et al. 2006). Exploring why managers attach such weight to the EPS impact of repurchases represents an important step toward a better understanding of this increasingly significant aspect of payout policy.

Recent research sheds light on the links between repurchase decisions and EPS-related considerations. Kahle (2002), Bens et al. (2002), and Bens et al. (2003) focus on the dilutive impact of employee stock options (ESOs). Their findings suggest repurchases are a managerial response to EPS dilution concerns. Evidence also suggests that managers use repurchases for benchmark-beating purposes, including meeting or exceeding analysts' EPS forecasts (Hribar et al. 2006), preserving a sequence of EPS improvement (Myers et al. 2007), and maintaining historic EPS growth rates (Bens et al. 2003).

Our analysis builds on prior research by examining whether managers' stock repurchase decisions are sensitive to explicit EPS-related incentives provided by executive compensation contracts. Compensation contracts linking rewards to EPS performance provide executives with direct and potentially powerful incentives to manage reported EPS. We therefore test whether repurchase activity is higher for firms with executive compensation tied to EPS performance.

Empirical tests employ data for a comprehensive sample of U.K. nonfinancial firms over the period January 1998 through December 2006. Several features make the U.K. a particularly attractive setting in which to explore the link between repurchases and compensation contract design. First, in addition to executive bonus plans that routinely condition rewards on EPS performance, executives' long-term incentives including options and restricted stock frequently employ EPS vesting conditions (Conyon et al. 2000; Carter et al. 2009). Second, regulatory restrictions governing the treatment of repurchases during the majority of our sample period help simplify empirical tests by tempering the link between repurchase activity and the dilutive effects of ESOs. In particular, U.K. Company Law required repurchased shares to be cancelled immediately rendering repurchases a costly device for offsetting ESO-related EPS dilution (because issuing new shares is administratively more costly than reissuing treasury stock). Instead, U.K. firms with ESO programs typically established a wholly owned trust company to repurchase (and reissue) shares on their behalf. Under U.K. GAAP, shares held by ESO trusts are excluded from the EPS calculation until shares vest unconditionally. Since ESO shares are purchased solely to fund share-based compensation plans and because ESO purchases do not meet the legal definition of a stock repurchase, our tests can distinguish between repurchases driven by dilution concerns (ESO shares) and repurchases driven by other factors.[1]

Findings reveal a significant association between repurchase activity and the presence of EPS-based compensation arrangements. The predicted odds of a repurchase for firms for which executive compensation depends on EPS performance are almost twice the level observed for firms for which rewards are independent of EPS. EPS conditions in bonus and option plans are associated with incrementally significant effects. Our findings suggest that EPS performance conditions represent an important determinant of U.K. managers' stock repurchase decisions.

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[1] U.K. Company Law was amended in December 2003 to allow firms to hold repurchased shares as treasury stock. Consistent with the absence of ESO-related motives for repurchases, none of our sample firms mentioned the dilutive impact of stock-based compensation plans among the list of repurchase reasons disclosed in their annual reports prior to this date. Following the regime switch, most U.K. firms continue to cancel repurchased shares. As described in Section III, we exclude from our subsequent empirical tests firms that explicitly repurchase stock into treasury post-December 2003 to fund option plans.

An important ancillary question raised by our findings is how the link between repurchases and executive compensation arrangements impacts shareholder value. We explore this issue by examining the costs and benefits associated with compensation-driven repurchases. On balance, our evidence suggests that EPS-driven repurchases yield net benefits to shareholders. First, repurchasers make higher aggregate payouts to shareholders regardless of the performance conditions employed in executive compensation contracts, and we find no evidence that EPS-driven repurchase payouts occur at the expense of either investment myopia (Bens et al. 2002) or dividend substitution (Grullon and Michaely 2002). Second, tests reveal several contracting benefits from repurchase incentives caused by linking compensation to EPS performance, including a stronger association between cash performance and repurchases in the presence of surplus cash, a higher propensity to signal undervaluation through a repurchase when stock price falls below intrinsic value, and lower accrual manipulation.

Our study contributes to prior research in several ways. First, while extant work links repurchases to the dilutive impact of employee option plans (Kahle 2002; Bens et al. 2002; Bens et al. 2003), and EPS-based bonus plans to the dilutive impact of new equity issues (Huang et al. 2010), the association between repurchase activity and EPS-contingent compensation has attracted little attention. Our study highlights EPS-based executive compensation contracts as an important determinant of repurchase activity that is entirely consistent with the EPS benefits of buybacks frequently highlighted by management (Badrinath and Varaiya 2000; Brav et al. 2005; Caster et al. 2006). Our findings complement and extend Marquardt et al.'s (2009) evidence that EPS-based bonus plans explain managers' preference for accelerated stock repurchases over regular open market buybacks. We add to their findings by demonstrating that EPS-based compensation arrangements also explain the underlying decision to repurchase stock and that shareholders benefit from this relation. Second, our analysis relates to work on performance measure choice in compensation contracts. In particular, prior research emphasizes how EPS targets encourage overinvestment (Brealey et al. 2008, 889). Our results provide a counterbalance to this view by highlighting how the repurchase incentives created by EPS-contingent pay help align managers' interests with those of shareholders. Our conclusion is consistent with Huang et al. (2010) who model the use of EPS conditions in executive bonus plans and find that EPS-contingent compensation helps resolve agency problems by protecting current shareholders from a reduction in proportional ownership. Our analysis also contributes to the small body of research exploring the interaction between alternative earnings management instruments (e.g., Demski et al. 2004; Cohen et al. 2008). We argue that shareholders gain when managers manipulate EPS through repurchases rather than accounting accruals, and we show that the incentive created by EPS targets to manipulate via repurchases correlates with less accrual management.

Section II develops the link between repurchases and executive compensation arrangements, and reviews the structure of executive compensation plans in the U.K. Section III provides details of our sample, data, and research design. Section IV reports results of tests that examine the association between repurchases and EPS-based performance targets in executive compensation plans. Section V explores the implications for shareholders of repurchases motivated by EPS-contingent performance conditions. Section VI concludes.

II. LITERATURE REVIEW AND INSTITUTIONAL OVERVIEW

Stock Repurchases, Earnings Management, and Executive Compensation

Despite being overlooked in much of the corporate payout policy literature, managers and financial commentators have long recognized the EPS implications of stock repurchases. The impact of repurchases on reported EPS represents the net of both numerator and denominator effects. The numerator effect, which works to reduce EPS, represents the decline in earnings caused by an increase in borrowing (for repurchases financed with debt) or a reduction in investment returns (for repurchases financed using cash reserves). The denominator effect serves to increase EPS by reducing the number of shares outstanding. Repurchases have a positive net effect on EPS for firms whose earnings-to-price ratio exceeds the opportunity cost of funds (i.e., either the after-tax return on short-term cash investments or the cost of debt; Guay 2002; Bens et al. 2003; Hribar et al. 2006). Survey evidence reported by Brav et al. (2005) highlights the central role EPS considerations play in shaping managers' repurchase decisions, with three-quarters of senior executives citing EPS growth as an important factor affecting their repurchase decisions.

Research has begun to explore the link between repurchases and EPS in several contexts. One EPS-related factor predicted to motivate repurchases is earnings dilution caused by ESO plans. Accretive stock repurchases can offset the dilutive effects of ESOs on reported EPS in several ways. For example, while ESO exercises reduce basic EPS by increasing the weighted average number of shares outstanding for the period, managers can mitigate this dilution by repurchasing shares to fund option exercises. Bens et al. (2002) and Kahle (2002) present evidence consistent with this option-funding hypothesis. Conversely, Bens et al. (2003) conclude that repurchases are not a response to the dilutive impact of option exercises on basic EPS. Instead, their results suggest the link between repurchases and options is driven by the effect of ESOs on diluted EPS, with buybacks increasing in the level of in-the-money ESOs outstanding.

Repurchases motivated by EPS considerations have also been linked with benchmark-beating earnings management activity. Controlling for dilution effects, Bens et al. (2003) find that repurchases are increasing in the amount by which earnings undershoot the level required to sustain historical diluted EPS growth. Myers et al. (2007) document similar behavior in a sample of firms characterized by long strings of consecutive quarterly EPS increases, with managers strategically timing repurchases to boost reported EPS when the string would otherwise be broken. Meanwhile, Hribar et al. (2006) conclude that managers use repurchases to meet or beat analysts' consensus EPS forecasts. In all these studies, benchmark-beating stock repurchase activity is motivated through implicit managerial incentives in the form of higher stock-related compensation, greater job security, and a lower cost of capital.[2]

Compensation contracts represent a powerful source of incentives for managers. For example, research demonstrates that executives use their accounting discretion to manipulate earnings in response to compensation-driven considerations (Bushman and Smith 2001). Further, a growing body of evidence suggests that corporate payout decisions are sensitive to executives' compensation arrangements. For instance, firms for which the executives' annual bonus pool is contingent on dividends paid are associated with higher dividend payouts and yields (White 1996), while ESOs that are not dividend-protected create incentives for executives to reduce dividend payments

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[2] Bens et al. (2003, 75–76) argue that explicit compensation contract considerations are not the source of their findings linking repurchases to EPS manipulation. They estimate firm-specific correlations between CEO cash compensation and reported EPS and use the median correlation to divide firms into high and low cash compensation-EPS sensitivity firms. Tests reveal no evidence that their main results differ across the two subsamples. However, this approach is unlikely to provide a powerful means of distinguishing firms with explicit EPS targets from those using alternative earnings-based performance metrics. Further, since the approach focuses exclusively on cash compensation, it ignores the impact of EPS vesting conditions in long-term compensation.

(Lambert et al. 1989; Kahle 2002; Fenn and Liang 2001). In related research, Aboody and Kasznik (2008) demonstrate how compensation plan design can help align managers' cash payout decisions with shareholders' tax-driven payout preferences. Finally, Wallace (1997) and Marquardt et al. (2009) examine the link between bonus plan performance conditions and corporate payout policy. Using a sample of firms adopting residual income-based plans (which penalize managers for accumulating capital that earns less than the opportunity cost of capital), Wallace (1997) documents a post-adoption rise in repurchase activity as managers liquidate unproductive assets. Marquardt et al. (2009), meanwhile, provide direct evidence on the link between EPS performance conditions and stock repurchase activity. Specifically, they find that managers are more likely to favor accelerated stock repurchases (which record the full EPS benefits of the repurchase immediately) over regular open market repurchases when their bonus plans are explicitly tied to EPS performance. However, the extent to which EPS-based compensation arrangements explain the propensity to repurchase more generally remains unexplored.

EPS is a popular performance metric used in executive compensation contracts (Murphy 1999; Conyon et al. 2000; Pass et al. 2000). Compensation contracts that tie managerial rewards to EPS create explicit incentives for executives to manage the EPS denominator through repurchases (over and above any implicit market-based incentives associated with increasing stock-based wealth and improving job security). These direct incentives are absent in compensation contracts that employ non-per-share-based earnings metrics such as return on assets, and nonaccounting measures such as stock price or qualitative targets linked to personal objectives. Accordingly, we predict that stock repurchase activity will be positively associated with the incidence of EPS-based performance conditions in executive compensation contracts. We test this prediction in the U.K. where short- and long-term elements of executive pay are linked to EPS (Conyon et al. 2000; Pass et al. 2000; Carter et al. 2009).

Overview of Executive Compensation Arrangements in the U.K.

The typical compensation package for a U.K. executive director includes both short-term bonus arrangements and longer-term incentives such as options and restricted stock (Conyon and Murphy 2000). Bonus payments are normally linked to short-term performance measures and objectives. In addition, U.K. firms regularly impose performance-vesting conditions in executives' long-term stock- and cash-based plans. Widespread adoption of performance-vesting conditions in executives' long-term compensation plans can be traced to the Greenbury Report (1995), which proposed that all long-term incentives (including option plans) should include challenging performance criteria. After December 31, 1995, revised London Stock Exchange rules required all listed firms to either comply with the Greenbury recommendation or publish a statement explaining noncompliance. Further pressure to adopt performance conditions for long-term incentives was applied by influential shareholder groups including the Association of British Insurers and the National Association of Pension Funds. As a result, performance vesting conditions in long-term plans are now commonplace among U.K. firms (Carter et al. 2009).

While best practice compensation guidelines do not favor any single performance metric, survey evidence reveals widespread adoption of EPS-based targets. For example, Conyon et al. (2000) report that 72 percent of option plans with performance-contingent vesting conditions define targets in terms of EPS growth, while Pass et al. (2000) find that 34 percent of long-term incentive plans (LTIPs) surveyed had an EPS performance condition. Accordingly, the performance conditions applied in long-term compensation arrangements often mirror those used in short-term bonus plans, for which EPS targets have long been used. The widespread use of EPS targets in executive compensation arrangements is expected to create powerful incentives for U.K. executives to manage EPS realizations through repurchases.

III. DATA AND METHODS

Sample and Data

The initial sampling frame comprises all U.K.-resident firms (excluding closed-end investment trusts) listed on the London Stock Exchange (LSE) with fiscal year-ends between January 1, 1998 and December 31, 2006. The sample period starts in 1998 because executive compensation data are collected with a one-year lag and disclosures relating to performance conditions in executive compensation contracts are patchy before 1997.

Firm-level repurchase data relate to aggregate reacquisitions made during a fiscal year. Only repurchases executed in the open market or via self-tender offer are used in subsequent tests. We hand-collect annual repurchase data from firms' published financial statements. This process involves identifying potential repurchasers using a variety of news sources including the London Stock Exchange Regulatory News Service, the Securities Data Corporation, and *The Financial Times*. Financial statements with year-ends between January 1998 and December 2006 are then examined for all firms in the provisional list to identify the number, value, and fraction of shares repurchased. The resulting sample comprises 1,047 repurchase firm-year observations for 460 firms. We remove financial firms due to the unique nature of performance measurement in that sector. Utility firms are also removed due to a lack of nonrepurchasing firms in the same sector for matching purposes (see below). A further 67 observations are lost due to missing data required to construct one or more of our test variables. We also exclude ten treasury stock repurchase observations driven entirely by outstanding option commitments. The final sample consists of 665 repurchase firm-years.

Repurchasing firms are drawn from 31 Datastream level-4 nonfinancial industry groups, with no single industry accounting for more than 12 percent of the final sample. The aggregate value of shares reacquired during the sample window exceeds £83 billion, with an average (median) annual repurchase value of £124.9 million (£3.9 million). Repurchase activity in the U.K. is increasing over time, with the aggregate amount rising from £636 million in 1998 to almost £28 billion in 2006. The average (median) annual repurchase involves approximately 5 (3) percent of common shares outstanding. Repurchased shares are cancelled in the majority of cases: only 66 observations (10 percent) utilize the treasury stock option.

Empirical tests require details of performance conditions used in executive compensation contracts, data for which are also hand-collected from firms' published annual reports and financial statements. Collecting such data for all LSE-listed nonrepurchase firms is infeasible. We therefore employ a case-control matched sample design whereby each of the 665 repurchase firm-year observations is paired with a time-, industry- and size-matched nonrepurchasing firm.[3] Matching by industry (Datastream level-4) helps control for factors that are expected to affect payout policy (Smith and Watts 1992) and compensation arrangements (Antle and Smith 1986), while matching by size (lagged total assets) helps to control for established associations between firm size and repurchase activity (Dittmar 2000; Jagannathan et al. 2000), and between firm size and compensation arrangements (Pass et al. 2000). Nonrepurchase control firms are matched with repurchasers at the fiscal year-end immediately preceding the repurchase year. Nonrepurchasers must not have implemented a buyback at any point prior to the matching year or during the subsequent four-year period.

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[3] Case-control matching unavoidably leads to disproportionate random sampling on the dependent variable. However, subsequent tests linking repurchase activity to EPS-based performance conditions in executive compensation contacts employ logistic regression, a well-known property of which is that slope coefficients remain unbiased in the presence of disproportionate random sampling on the dependent variable (Prentice and Pyke 1979).

Details of the following performance-related elements of executive compensation are collected from repurchase and nonrepurchase firms' annual reports in the matching year: short-term bonus plans, option plans, and long-term incentive plans. Bonus plans comprise all arrangements for which rewards are tied to short-term (≤one-year) performance targets. Option plans comprise all stock-based arrangements granting executives the right to acquire shares at a nonzero exercise price. (Firm-wide employee option plans and save-as-you-earn schemes are excluded.) LTIPs consist of all remaining long-term compensation arrangements not classified as options (e.g., deferred bonus schemes, share matching schemes, zero strike price options, stock appreciation rights, long-term cash-based bonus plans, etc.). We record the performance conditions for all active plans in each category. The data-collection process has to confront two disclosure problems. First, a handful of firms fail to unambiguously disclose use of one or more of the three plan types. Second, some firms fail to provide details of the performance conditions used in one or more of their plans. We use previous years' Annual General Meeting resolutions and remuneration disclosures up to two years ahead to verify plan existence and determine performance conditions employed.[4] Cases for which we cannot unambiguously determine plan existence or the use of an EPS performance condition are coded as nondisclosers.

Research Design

We expect EPS performance conditions to be more prevalent among repurchasing firms. We test this prediction using a conditional logistic regression to model the probability of a repurchase and a left-censored tobit regression to model the value of repurchases:

$$Log\left(\frac{p_{it}}{1-p_{it}}\right) = \gamma_1 NDISC_{ijt-1} + \gamma_2 EPS_{ijt-1} + \sum_{k=1}^{K} \theta_k Controls_{kit-1}, \tag{1}$$

$$Rep_{it} = \lambda_0 + \lambda_1 NDISC_{ijt-1} + \lambda_2 EPS_{ijt-1} + \sum_{k=1}^{K} \delta_k Controls_{kit-1} + \upsilon_{it}. \tag{2}$$

For the conditional logistic model presented in Equation (1), p_{it} is the latent probability that firm i repurchases shares in year t ($y_{it} = 1$). The conditional logistic model is the appropriate estimation method for the matched pair structure of our data (Allison 1999, 203).[5] For the left-censored tobit model presented in Equation (2), Rep is the observed value of the latent propensity to repurchase stock (Rep^*): $Rep_{it} = 0$ if $\mathrm{Rep}^*_{it} \leq 0$ and Rep_{it} = value of stock repurchases in fiscal year t scaled by lagged total assets if $Rep^*_{it} > 0$.

The vector of explanatory variables is the same in Equations (1) and (2): *NDISC* is an indicator variable taking the value of 1 if the presence of an EPS performance condition is indeterminate for at least one of the j compensation components (j = bonus plans, stock option plans, and LTIPs), and 0 otherwise; *EPS* is an indicator variable taking the value of 1 if an EPS performance condition is used in at least one of the j compensation components, and 0 otherwise; and *Controls* is a vector of K additional factors expected to influence the repurchase decision.[6] The set of control variables includes the market-to-book ratio, net leverage, dividend yield, prior-period abnormal stock price performance, and firm size (Stephens and Weisbach 1998; Dittmar 2000; Grullon and Michaely 2002). Since repurchases have a positive net effect on EPS when a firm's earnings-to-price (E/P) ratio exceeds its opportunity cost of funds, only firms that meet such a condition will repurchase shares to boost EPS. We therefore include an indicator variable for firms reporting negative earnings on the grounds that the E/P condition is least likely to hold in such cases. Prior research documents a link between ESO plans and stock repurchases by U.S. firms (Fenn and Liang 2001; Dittmar 2000). Although U.K. regulatory rules governing stock repurchases militate against such behavior during our sample period, for completeness we include the total number of options outstanding for all employees (scaled by market capitalization) as an additional control variable. To ensure that our analysis is capturing effects unique to EPS, we also control for the presence of earnings-based performance conditions of any description.[7] Finally, we control for the well-established link between repurchases and surplus cash (Stephens and Weisbach 1998; Dittmar 2000; Guay and Harford 2000; Jagannathan et al. 2000). The presence of surplus cash is captured using both stock (surplus cash holdings) and flow (excess cash flow) measures. Our measure of surplus cash holdings is cash and cash equivalents in excess of the level required for normal operations and investments. Following Opler et al. (1999), we estimate surplus cash holdings as the residual from an OLS regression of cash holdings (scaled by lagged noncash assets) on a vector of explanatory variables comprising the market-to-book ratio, net working capital (scaled by lagged total assets), lagged operating cash flow (scaled by lagged total assets), net leverage, R&D spending (scaled by total revenue), the natural logarithm of market capitalization, an indicator variable for nonzero dividend payments, and industry fixed effects. The regression is estimated annually using all Extel nonfinancial firms with complete data after excluding the top and bottom percentiles of scaled cash holdings. We extract the residuals (ε_{it}) from the annual estimations to construct an indicator variable equal to 1 when $\varepsilon_{it} > 0$, and 0 otherwise.

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[4] Compensation disclosures in the U.K. improved dramatically during the late 1990s and early 2000s. When using one- and two-period-ahead remuneration disclosures, we are careful to distinguish between established plans and new plans introduced subsequently.

[5] We also estimated Equation (1) using a pooled (unmatched) logistic model with very similar results. Results are available from the authors on request.

[6] Incomplete disclosure of performance conditions means that EPS realizations may take one of three forms: EPS condition is used and disclosed; EPS condition is unambiguously not used; and EPS condition is indeterminate due to insufficient disclosure. Defining *EPS* in Equations (1) and (2) as a three-way categorical variable imposes a linearity constraint on the data. The alternative (unconstrained) approach is to recode the three-way variable as three separate

We use two measures of excess cash flow, one based on operating activities (*Free cash flow*) and one based on nonoperating activities (*Excess investing cash flow*). Following Opler and Titman (1993) and Fenn and Liang (2001), *Free cash flow* is an indicator variable equal to 1 for firms with a market-to-book ratio less than the Extel annual sample median and operating cash flow (scaled by lagged total assets) greater than the Extel annual sample median, and 0 otherwise. *Excess investing cash flow* is an indicator variable taking the value of 1 when net cash inflow from investing activities is positive, and 0 otherwise. (Investing cash inflows result from the sale of fixed and intangible assets, associates and other investments, and subsidiaries.) All explanatory variables in Equations (1) and (2) are measured at the start of the repurchase year.

IV. RESULTS

Descriptive Statistics

Table 1, Panel A reports descriptive statistics for the incidence of bonus plans, option plans, and LTIPs. Frequency counts reported in columns 2–4 reveal most firms operate at least one bonus plan and one option plan, whereas only 40 percent of firms have an active LTIP. Cross-sample comparisons indicate that repurchasers are marginally more likely to operate a bonus plan ($p = 0.09$). Consistent with the absence of powerful ESO-related motives for stock repurchases in

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dummy variables and then use two of these in place of the original variable (Allison 1999, 128–130). Tests reveal that imposing the linearity constraint on our data leads to a reduction in model fit (the change in the likelihood ratio statistic is significant at the 0.01 level), suggesting that the unconstrained formulation presented in Equations (1) and (2) is more appropriate in our case.

[7] For example, residual income also creates incentives to distribute capital earning less than the opportunity cost of funds (Wallace 1997).

TABLE 1

Summary Statistics and Features of Compensation Plan Components for Repurchaser and Nonrepurchaser Matched Pairs

Panel A: Frequency of Plans

	Plan Status by Firm			Number of Plans				
Compensation Component	**≥1 Plan**	**No Plan**	**Not Disclosed**	**n**	**Mean**	**Std. Dev.**	**Median**	**Max**
Bonus Plans								
Repurchasers	601	49	15	605	0.931	0.277	1	2
Nonrepurchasers	596	68	1	600	0.904	0.315	1	2
p-value for difference	0.09				0.17		0.17	
Option Plans								
Repurchasers	591	69	5	759	1.150	0.621	1	3
Nonrepurchasers	601	64	0	789	1.186	0.620	1	3
p-value for difference	0.62				0.29		0.31	
Long-Term Incentive Plans								
Repurchasers	282	377	6	377	0.572	0.745	0	3
Nonrepurchasers	287	378	0	373	0.561	0.744	0	3
p-value for difference	0.89				0.77		0.74	

Panel B: Performance Measures by Plan

	Frequency Counts			
	Repurchasers		Nonrepurchasers	
Performance Measures by Compensation Component	**n**	**%**	**n**	**%**
Bonus Plans				
Earnings per share	168	(27.8)	121	(20.2)
Profit before tax/EBIT/Operating profit	303	(50.1)	329	(54.8)
Residual income	15	(2.5)	7	(1.2)
Return on capital	21	(3.5)	34	(5.7)
Share price/Total shareholder return	16	(2.6)	16	(2.7)
Personal objectives	88	(14.5)	118	(19.7)
Other	198	(32.7)	252	(42.0)
Not disclosed	153	(25.3)	122	(20.3)
Total Number of Plans	605		600	
Option Plans				
Earnings per share	488	(64.3)	398	(50.4)
Profit before tax/EBIT/Operating profit	24	(3.2)	15	(1.9)
Return on capital	4	(0.5)	1	(0.1)
Share price/Total shareholder return	62	(8.2)	125	(15.8)
Personal objectives	0	(0.0)	1	(0.1)
Other	8	(1.1)	12	(1.5)
No performance condition	176	(23.2)	238	(30.2)
Not disclosed	27	(3.6)	9	(1.1)
Total Number of Plans	759		789	
Long-Term Incentive Plans				
Earnings per share	155	(41.1)	151	(40.5)
Profit before tax/EBIT/Operating profit	13	(3.4)	17	(4.6)

(continued on next page)

Panel B: Performance Measures by Plan

Performance Measures by Compensation Component	Repurchasers n	Repurchasers %	Nonrepurchasers n	Nonrepurchasers %
Residual income	6	(1.6)	2	(0.5)
Return on capital	10	(2.7)	13	(3.5)
Share price/Total shareholder return	217	(57.6)	196	(52.5)
Other	14	(3.7)	17	(4.6)
No performance condition	52	(13.8)	43	(11.5)
Not disclosed	1	(0.3)	3	(0.8)
Total Number of Plans	377		373	

The sample comprises 665 fiscal years between January 1, 1998 and December 31, 2006 in which firms repurchased shares and 665 nonrepurchase firm-years matched by fiscal year, industry, and lagged total assets.

All compensation data relate to compensation contracts for executive directors. Data are collected on all plans for the following three elements in executives' compensation contracts: bonuses, share options, and long-term incentives. Bonus plans comprise all arrangements where rewards are tied to short-term (≤one-year) performance targets. Option plans consist of incentive contracts granting executives the right to acquire their firm's shares a non-zero exercise price. (Firm-wide employee share option plans and save-as-you-earn schemes are not included.) Long-term incentive plans (LTIPs) consist of all remaining long-term compensation arrangements not classified as options. Probability values reported in Panel A are for Chi-square tests (column 2), paired t-tests (column 6), and paired Wilcoxon tests (column 8).

For each compensation component, the sum of performance measure percentages reported in Panel B may exceed 100 because some firms use multiple measures in a single plan.

the U.K., the frequency of firms with at least one option plan is equivalent in the two samples, as is the incidence of LTIPs. The final three columns in Panel A report summary statistics for the number of active plans. No significant differences are apparent between the two samples.

Plan-level details of performance conditions are reported in Table 1, Panel B. For bonus plans, 28 percent of the 605 plans operated by repurchasers have EPS performance conditions, as compared to only 20 percent of 600 comparable plans operated by nonrepurchasers. In contrast, aggregate profit-based metrics such as profit before tax, personal objectives, and other measures (e.g., operations metrics, KPIs) are more common among nonrepurchasers. Note also that repurchase firms are characterized by poorer disclosure of bonus-related performance conditions: 25 percent of plans in the repurchase sample contain no details of performance conditions, as compared to only 20 percent of plans in the nonrepurchase sample. Similar patterns are apparent for option plans. Option exercise is conditional on EPS performance in 64 percent of repurchase firms' plans, as compared to 50 percent of plans in the nonrepurchase sample. Results for bonus and option plans provide preliminary evidence that repurchase activity is increasing in the presence of EPS performance conditions. No difference in the use of EPS conditions exists for LTIPs; roughly 40 percent of plans in both samples are conditional on EPS performance.

Summary statistics for our main test variables are reported in Table 2. Seventy-one percent of repurchasers have at least one plan linking at least one element of executive compensation to EPS. The comparable figure for nonrepurchasers is 60 percent, which is significantly lower based on a paired Wilcoxon test ($p < 0.01$). Analyzing the incidence of EPS targets for each compensation element separately reveals that repurchasers are significantly more likely to have at least one bonus plan and at least one option plan tied to EPS. In contrast, repurchase and nonrepurchase firms are equally likely to have an LTIP conditional on EPS. Significant differences across repur-

TABLE 2

Descriptive Statistics

	Repurchase Sample (n = 665)						
	Mean	**Std. Dev.**	**Max**	**Q3**	**Median**	**Q1**	**Min**
Compensation Variables							
EPS	0.71	0.46	1	1	1	0	0
$NDISC$	0.14	0.35	1	0	0	0	0
EPS_{Bonus}	0.25	0.43	1	1	0	0	0
$NDISC_{Bonus}$	0.24	0.43	1	0	0	0	0
EPS_{Option}	0.62	0.49	1	1	1	0	0
$NDISC_{Option}$	0.05	0.21	1	0	0	0	0
EPS_{LTIP}	0.20	0.40	1	0	0	0	0
$NDISC_{LTIP}$	0.01	0.09	1	0	0	0	0
Control Variables							
Surplus cash holdings	0.69	0.46	1	1	1	0	0
Free cash flow	0.32	0.47	1	1	0	0	0
Excess investing cash flow	0.19	0.39	1	0	0	0	0
Log(market capitalization)	5.29	2.43	11.71	6.96	5.05	3.30	−0.21
Market-to-book	1.61	0.96	6.29	1.88	1.31	1.00	0.44
Net leverage	−0.15	1.54	0.84	0.19	0.05	−0.15	−24.61
Dividend yield	0.04	0.07	1.42	0.05	0.03	0.02	0.00
Negative returns	0.56	0.50	1	1	1	0	0
Negative earnings	0.10	0.30	1	0	0	0	0
Options outstanding	0.76	18.38	47.91	0.04	0.01	0.00	0.00

	Nonrepurchase Sample (n = 665)							
	Mean	**Std. Dev.**	**Max**	**Q3**	**Median**	**Q1**	**Min**	**p-value for Difference**
Compensation Variables								
EPS	0.60	0.49	1	1	1	0	0	0.01
$NDISC$	0.09	0.29	1	0	0	0	0	0.01
EPS_{Bonus}	0.18	0.39	1	0	0	0	0	0.01
$NDISC_{Bonus}$	0.18	0.38	1	0	0	0	0	0.01
EPS_{Option}	0.50	0.50	1	1	1	0	0	0.01
$NDISC_{Option}$	0.01	0.10	1	0	0	0	0	0.01
EPS_{LTIP}	0.19	0.40	1	0	0	0	0	0.61
$NDISC_{LTIP}$	0.01	0.08	1	0	0	0	0	0.75
Control Variables								
Surplus cash holdings	0.65	0.48	1	1	1	0	0	0.09
Free cash flow	0.29	0.45	1	1	0	0	0	0.16
Excess investing cash flow	0.14	0.35	1	0	0	0	0	0.02
Log(market capitalization)	4.72	1.91	10.74	6.06	4.73	3.20	−0.88	0.01
Market-to-book	1.63	1.61	29.69	1.69	1.25	1.00	0.41	0.01
Net leverage	0.03	0.88	1.87	0.29	0.15	−0.02	−10.69	0.01
Dividend yield	0.03	0.09	1.46	0.04	0.02	0.00	0.00	0.01
Negative returns	0.61	0.49	1	1	1	0	0	0.01
Negative earnings	0.28	0.45	1	1	0	0	0	0.01
Options outstanding	0.16	0.77	11.24	0.07	0.02	0.01	0.00	0.01

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All variables are measured at the beginning of the repurchase year. The "p-value for Difference" column reports probability values for two-tailed paired Wilcoxon Sign Rank tests of the difference between repurchase and nonrepurchase firms.

Variable Definitions (with Extel codes in square brackets where applicable):

NDISC = indicator variable taking the value of 1 when insufficient disclosure renders the presence of an EPS target indeterminate, and 0 otherwise;
EPS = indicator variable taking the value of 1 for firms with at least one bonus plan, option plan, or LTIP tied to EPS performance, and 0 otherwise;
$NDISC_{Bonus}$ = indicator variable taking the value of 1 for firms that fail to explicitly disclose whether bonus payments are conditional on EPS performance, and 0 otherwise;
EPS_{Bonus} = indicator variable taking the value of 1 for firms where bonus payments are fully or partially conditional on EPS performance, and 0 otherwise;
$NDISC_{Option}$ = indicator variable taking the value of 1 for firms that fail to explicitly disclose whether option vesting is conditional on EPS performance, and 0 otherwise;
EPS_{Option} = indicator variable taking the value of 1 for firms where option vesting is fully or partially conditional on EPS performance, and 0 otherwise;
$NDISC_{LTIP}$ = indicator variable taking the value of 1 for firms that fail to explicitly disclose whether LTIP rewards are conditional on EPS performance, and 0 otherwise;
EPS_{LTIP} = indicator variable taking the value of 1 for firms where LTIP rewards are fully or partially conditional on EPS performance, and 0 otherwise;
Surplus cash holdings = indicator variable based on the residual from yearly OLS regressions of the natural logarithm of cash holdings (cash and cash equivalents [ex.CurrentAssetsCashAndNearCash + ex.CurrentAssetsCurrentInvestments] scaled by lagged total assets [ex.TotalAssets] net of cash and cash equivalents) on the natural logarithm of market capitalization [ex.MarketCapitalization], operating cash flow [ex.CFOperatingInflows] scaled by lagged total assets, net working capital (noncash current assets [ex.CurrentAssets − ex.CurrentAssetsCashAndNearCash − ex.CurrentAssetsCurrentInvestments] − current liabilities [ex.Creditors + ex.DebtSTLoans] divided by total assets net of cash and cash equivalents), net leverage (total liabilities [ex.DebtLTLoans + ex.DebtSTLoans] net of cash and cash equivalents divided by total assets net of cash and cash equivalents), research and development ([ex.TradingExpResearchAndDevelopment] divided by total revenue [ex.Sales]), the market-to-book ratio (book value of debt [ex.TotalAssets − ex.ShareholdersEquityOwnEquShares − ex. ShareholdersEquityOwnSharePrem − ex.ShareholdersEquityPreferShares − ex.ShareholdersEquityParticipShares] plus the market value of equity divided by total assets), and dividend payout (an indicator variable taking the value of 1 for firms with non-zero ordinary dividends per share [ex.DividendsPerShareNetReported], and 0 otherwise); *Surplus cash holdings* indicator variable takes the value of 1 where the regression residual is positive, and 0 otherwise;
Free cash flow = indicator variable taking the value of 1 for firms with a market-to-book ratio less than the entire Extel annual sample median and net operating cash flow (scaled by lagged total assets) greater than the entire Extel annual sample median, and 0 otherwise;
Excess investing cash flow = indicator variable taking the value of 1 if investing cash flows [ex.CFInvestments] are positive, and 0 otherwise;
Market capitalization = fiscal year-end share price multiplied by the number of shares outstanding;
Market-to-book = book value of debt plus the market value of equity divided by total assets;
Net leverage = total liabilities net of cash and cash equivalents divided by total assets net of cash and cash equivalents;
Dividend yield = ordinary dividends per share divided by share price;
Negative returns = indicator variable taking the value of 1 if 12-month stock returns are less than the market return over the corresponding period, and 0 otherwise;
Negative earnings = indicator variable taking the value of 1 if earnings per share [ex.EPSAsReported] is negative, and 0 otherwise; and
Options outstanding = aggregate number of outstanding options for all employees at the balance sheet date scaled by market capitalization.

chase and control samples are apparent for almost all control variables. Consistent with prior research, repurchasers have more surplus cash, lower net leverage, and are larger than their nonrepurchaser counterparts.

Logistic and Tobit Regressions

Coefficient estimates and model summary statistics for conditional logistic regressions relating the probability of a repurchase to the incidence of EPS-based performance conditions are reported in Table 3, Models 1–3. Results for comparable left-censored tobit regressions are reported in Models 4–5. *EPS* in Model 1 equals 1 when at least one plan of any type links executive compensation to EPS, and 0 otherwise. As predicted, the estimated coefficient on *EPS* is positive and significant. EPS performance conditions also represent an economically important driver of repurchase activity; the predicted odds of a repurchase for firms where executive compensation depends on EPS performance are almost twice the odds for firms where payouts are independent of EPS. Note also that the odds ratio for *EPS* in Model 1 is similar to (and in many cases larger than) the odds ratios associated with traditional determinants of repurchase activity such as excess cash flow and low leverage.

Model 1 also reveals that repurchasers are less likely to disclose details of performance conditions used in executive compensation contracts. The estimated coefficient on *NDISC* is positive and highly significant, while the odds ratio is large relative to other variables. We have no predictions concerning the link between repurchase activity and the transparency with which firms disclose details of compensation arrangements. To ensure that nondiscloser cases are not confounding the analysis, we re-estimated Model 1 after removing such cases. Results reported in Model 2 are entirely consistent with those in Model 1. Whatever effect *NDISC* may be capturing, it appears to be distinct from our main prediction.

Of the remaining control variables in Models 1 and 2, coefficient estimates on *Free cash flow, Excess investing cash flow*, *Market-to-book, Net leverage*, and *Negative earnings* are statistically significant and of the predicted sign. In addition, even after controlling for differences in lagged total assets via our matching procedure, repurchasers are characterized by higher market capitalization at the beginning of the repurchase year.

Model 3 in Table 3 provides evidence on the incremental effects of EPS-based bonus, option, and LTIP arrangements. EPS-based bonus and option plans are associated with incremental positive effects of similar magnitude. In contrast, EPS-based LTIPs have no discernable effect on repurchase activity. The absence of an LTIP effect likely reflects the lower incidence of such plans relative to bonus and option plans (see Table 1, Panel A), coupled with the dominance of total shareholder return (TSR) performance conditions in these plans.[8]

Models 4 and 5 in Table 3 are for left-censored tobit regressions modeling the scaled value of repurchases. Findings replicate those for the conditional logistic models. Even after controlling for traditional determinants of repurchases, annual spending on buybacks is positively associated with *EPS* and the marginal effect for this variable is similar to (and in many cases larger than) the effect for more established drivers of repurchase activity. Consistent with the logistic results, while the coefficient on *NDISC* is positive and highly significant in Model 4, exclusion of nondisclosing matched pairs (Model 5) does not affect the *EPS* coefficient estimate. Collectively, results reported

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[8] Although many firms employ EPS targets in their LTIPs, these are often combined with (and subordinate to) a TSR condition. For example, awards under KBC Advanced Technologies' share-based LTIP require minimum TSR over a three-year period equal to the median for the FTSE Small Cap Index over the same period, with maximum awards for TSR at or above 75th percentile ranking. Irrespective of TSR performance, awards are conditional on real EPS growth over the performance period (KBC Advanced Technologies plc Annual Report 2002, 15–16).

TABLE 3

Coefficient Estimates and Model Summary Statistics for Conditional Logistic (Left-Censored Tobit) Regressions Relating the Probability (Scaled Value) of a Repurchase to the Incidence of EPS-Based Performance Conditions in Executive Compensation Contracts

Variables	Predicted Sign	Logistic Models			Tobit Models	
		Model 1	Model 2	Model 3	Model 4	Model 5
Surplus cash holdings	(+)	0.20	0.24	0.24	0.01	0.02
		1.22	*1.27*	*1.27*	*0.00*	*0.00*
		(0.18)	(0.17)	(0.12)	(0.01)	(0.02)
Free cash flow	(+)	0.69	0.60	0.66	0.00	0.00
		2.00	*1.83*	*1.93*	*0.00*	*0.00*
		(0.01)	(0.01)	(0.01)	(0.61)	(0.77)
Excess investing cash flow	(+)	0.71	0.86	0.75	0.02	0.03
		2.03	*2.40*	*2.11*	*0.01*	*0.01*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log(market capitalization)	(?)	1.27	1.51	1.22	0.00	0.00
		3.56	*4.52*	*3.39*	*0.00*	*0.00*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Market-to-book	(−)	−0.72	−0.81	−0.68	0.01	0.01
		0.49	*0.45*	*0.51*	*0.00*	*0.00*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Net leverage	(−)	−0.43	−0.46	−0.43	−0.01	−0.01
		0.65	*0.63*	*0.65*	*−0.00*	*−0.00*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Dividend yield	(?)	1.18	1.34	1.15	0.01	0.00
		3.25	*3.82*	*3.15*	*0.00*	*0.00*
		(0.20)	(0.17)	(0.23)	(0.80)	(0.93)
Negative returns	(+)	−0.05	−0.06	−0.03	0.00	0.00
		0.95	*0.94*	*0.97*	*0.00*	*0.00*
		(0.73)	(0.73)	(0.84)	(0.99)	(0.98)
Negative earnings	(−)	−1.22	−1.38	−1.14	−0.05	−0.06
		0.30	*0.25*	*0.32*	*−0.01*	*−0.01*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Options outstanding	(+)	−0.021	−0.03	−0.02	−0.00	−0.00
		0.98	*0.98*	*0.98*	*−0.00*	*−0.00*
		(0.10)	(0.13)	(0.13)	(0.04)	(0.07)
Earnings-based target	(?)	−0.15	−0.22	−0.06	−0.01	−0.01
		0.86	*0.80*	*0.94*	*−0.00*	*−0.00*
		(0.33)	(0.21)	(0.74)	(0.28)	(0.34)
NDISC	(?)	1.29			0.04	
		3.62			*0.01*	
		(0.01)			(0.01)	
EPS	(+)	0.65	0.71		0.03	0.03
		1.91	*2.03*		*0.01*	*0.01*
		(0.01)	(0.01)		(0.01)	(0.01)
$NDISC_{Bonus}$	(?)			0.54		
				1.72		
				(0.02)		

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TABLE 3 (continued)

Variables	Predicted Sign	Logistic Models			Tobit Models	
		Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
EPS_{Bonus}	(+)			0.36		
				1.43		
				(0.07)		
$NDISC_{Option}$	(?)			1.95		
				7.03		
				(0.01)		
EPS_{Option}	(+)			0.38		
				1.46		
				(0.02)		
$NDISC_{LTIP}$	(?)			−0.80		
				0.45		
				(0.38)		
EPS_{LTIP}	(+)			−0.08		
				0.92		
				(0.67)		
Intercept	(?)				−0.08	−0.08
					(0.01)	(0.01)
Likelihood ratio		278.44	233.59	283.14	328.63	276.65
p-value		0.01	0.01	0.01		
Pseudo R^2		0.46	0.48	0.46		
% classified correctly		77.30	77.70	75.60		
n		665	524	665	1330	1173
n censored					665	602

Three values are reported for each covariate: the first value is the coefficient estimate; the second (italicized) value is the odds ratio (marginal effect) for logistic (tobit) models; and the third value (in parentheses) is the two-tailed probability value.

The dependent variable in the columns headed "Logistic Models" is the logarithm of the odds of a repurchase. The dependent variable in the columns headed "Tobit Models" is the aggregate amount (including expenses) spent repurchasing shares during fiscal year *t*, scaled by lagged total assets. All variables are measured at the beginning of the repurchase year. Marginal effects for the tobit models are equal to the probability of a no-limit observation multiplied by the coefficient estimate and are evaluated at the mean of each covariate.

Variable Definitions:

NDISC = indicator variable taking the value of 1 when insufficient disclosure renders the presence of an EPS target indeterminate, and 0 otherwise;

EPS = indicator variable taking the value of 1 for firms with at least one of the *j* compensation components (*j* = bonuses, options, or LTIPs) linked to EPS performance, and 0 otherwise;

$NDISC_j$ = indicator variable taking the value of 1 for firms that fail to explicitly disclose whether the *j*th compensation element is conditional on EPS performance, and 0 otherwise;

EPS_j = indicator variable taking the value of 1 for firms where the *j*th compensation element is conditional on EPS performance, and 0 otherwise;

Surplus cash holdings = indicator variable taking the value of 1 where the residual from yearly optimal cash holdings regressions is positive, and 0 otherwise (see Table 2 for details);

Free cash flow = indicator variable taking the value of 1 for firms with a market-to-book ratio less than the sample median for the year and net operating cash flow greater than the sample median for the year, and 0 otherwise;

Excess investing cash flow = indicator variable taking the value of 1 if investing cash flows are positive, and 0 otherwise;

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TABLE 3 (continued)

Market capitalization = share price multiplied by the number of shares outstanding;
Market-to-book = book value of debt plus the market value of equity divided by total assets;
Net leverage = total liabilities net of cash holdings divided by total assets net of cash holdings;
Dividend yield = ordinary dividends per share divided by share price;
Negative returns = indicator variable taking the value of 1 if 12-month stock returns are less than the market return over the corresponding period, and 0 otherwise;
Negative earnings = indicator variable taking the value of 1 if reported EPS is negative, and 0 otherwise;
Options outstanding = aggregate number of outstanding options for all employees at the balance sheet date scaled by market capitalization; and
Earnings-based target = indicator variable equal to 1 for firms where at least one of the j compensation components is linked to any earnings-based metric (including EPS), and 0 otherwise.

in Table 3 provide consistent evidence that EPS-based performance conditions in executive compensation contracts are an important determinant of repurchase activity among U.K. firms.[9]

V. FURTHER ANALYSIS

While tests presented in the previous section establish an association between repurchase activity and performance conditions applied in executive compensation contracts, they leave unresolved the more fundamental question of whether such a link is in shareholders' best interests. This section seeks evidence on the costs and benefits associated with compensation-driven repurchase activity. We begin by testing whether repurchasers with EPS targets exhibit weaker performance (lower payouts to shareholders) as a consequence of management diverting funds from profitable investment opportunities (regular dividends) to fund repurchase activity. Next, we test whether compensation-driven repurchases help to alleviate the problems of surplus cash, suboptimally low leverage, market mispricing, and accrual manipulation.

Post-Repurchase Performance and Payouts

Bens et al. (2002) conclude that U.S. managers divert capital from positive NPV investments to fund repurchases aimed at offsetting the dilutive impact of ESOs. If managers adopt the same strategy to achieve EPS performance targets, then EPS-induced repurchases could represent a serious cost for shareholders in the form of underinvestment. Accordingly, we examine whether a higher probability of repurchases in the presence of EPS-based compensation arrangements is associated with material underinvestment problems.

All else equal, failure to exploit profitable investment opportunities by channeling capital from positive NPV projects to fund repurchases should be reflected in inferior future performance. Tests reported in Table 4 compare the performance of repurchasers and nonrepurchasers using a difference-in-differences OLS specification in which the dependent variable is the change in performance from the pre- to the post-repurchase period. We estimate models for two alternative performance metrics: change in return on assets (ΔROA) and change in market-adjusted stock returns ($\Delta Returns$). The vector of explanatory variables includes indicator variables for repurchas-

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[9] In supplementary tests we explored whether the propensity for repurchases is increasing in the number of compensation elements linked explicitly to EPS. The indicator variable EPS_{Single} (EPS_{Multi}) captures firm-years where one (more than one) compensation element is tied to EPS. For conditional logistic regressions, coefficient estimates on EPS_{Single} and EPS_{Multi} are 0.60 and 0.79, respectively, and significant at the $p < 0.01$ level. For tobit regressions, coefficient estimates on both covariates are equal to 0.03 and statistically significant at the $p < 0.01$ level. Results indicate that repurchases are more likely for firms with single and multiple components of pay tied to EPS. However, tests fail to reject equality of coefficient estimates in favor of the alternative hypothesis that the coefficient on $EPS_{Multi} > EPS_{Single}$.

TABLE 4

Coefficient Estimates and Model Summary Statistics for Difference-in-Differences OLS Regressions of Change in Performance Relative to Repurchase Year *t*

	Δ*Performance*: Pre- to Post-Repurchase				**Δ*Performance***	
	Δ*ROA*		**Δ*Returns***		**(*t*−1 to *t*+1) for:**	
Variable	***t*−1 to *t*+1**	***t*−1 to *t*+2**	***t*−1 to *t*+1**	***t*−1 to *t*+2**	**Δ*ROA***	**Δ*Returns***
ROA	−0.32	−0.29	−0.17	−0.04	−0.32	−0.17
	(0.01)	(0.01)	(0.04)	(0.63)	(0.01)	(0.05)
Negative earnings	−0.02	−0.01	0.05	−0.03	−0.02	0.04
	(0.06)	(0.50)	(0.23)	(0.45)	(0.05)	(0.31)
Returns	0.01	0.01	−0.90	−0.94	0.01	−0.90
	(0.15)	(0.09)	(0.01)	(0.01)	(0.14)	(0.01)
Log(Total assets)	0.01	0.01	0.01	−0.01	0.01	0.01
	(0.01)	(0.01)	(0.37)	(0.47)	(0.01)	(0.17)
Δ*Log(Total assets)*	0.04	0.04	0.20	0.15	0.04	0.19
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Market-to-book	−0.01	−0.01	−0.04	−0.02	−0.01	−0.04
	(0.02)	(0.01)	(0.01)	(0.13)	(0.01)	(0.01)
Leverage	−0.01	−0.04	−0.11	−0.04	−0.01	−0.13
	(0.78)	(0.14)	(0.19)	(0.71)	(0.78)	(0.14)
Working capital	0.00	0.01	−0.01	−0.01	0.00	−0.01
	(0.79)	(0.86)	(0.01)	(0.01)	(0.75)	(0.01)
Investing cash flow	0.02	0.00	−0.02	−0.02	0.03	0.09
	(0.43)	(0.92)	(0.90)	(0.88)	(0.38)	(0.48)
Operating cash flow	0.11	0.03	−0.02	−0.01	0.11	0.05
	(0.01)	(0.48)	(0.87)	(0.94)	(0.01)	(0.75)
Cash holdings	−0.03	−0.06	0.02	−0.24	−0.03	0.04
	(0.35)	(0.07)	(0.88)	(0.06)	(0.39)	(0.73)
Options outstanding	−0.00	−0.00	−0.01	0.01	0.01	−0.01
	(0.39)	(0.42)	(0.01)	(0.94)	(0.61)	(0.01)
Repurchase	−0.01	−0.01	0.11	0.09		
	(0.37)	(0.66)	(0.04)	(0.10)		
EPS	0.01	0.00	0.08	0.00		
	(0.23)	(0.86)	(0.06)	(0.97)		
Repurchase × *EPS*	0.02	0.01	−0.09	−0.04		
	(0.29)	(0.43)	(0.14)	(0.56)		
$Repurchase_{Cash\ rich}$					−0.04	−0.12
					(0.16)	(0.29)
$Repurchase_{Cash\ constrained}$					−0.02	0.13
					(0.14)	(0.03)
$Nonrepurchase_{Cash\ rich}$					−0.01	−0.01
					(0.49)	(0.86)
$Repurchase_{Cash\ rich} \times EPS$					0.05	0.07
					(0.09)	(0.54)
$Repurchase_{Cash\ constrained} \times EPS$					0.03	−0.10
					(0.03)	(0.12)
$Nonrepurchase_{Cash\ rich} \times EPS$					0.00	0.02
					(0.83)	(0.71)

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TABLE 4 (continued)

	Δ*Performance*: Pre- to Post-Repurchase				**Δ*Performance***	
	Δ*ROA*		**Δ*Returns***		**(t−1 to t+1) for:**	
Variable	**t−1 to t+1**	**t−1 to t+2**	**t−1 to t+1**	**t−1 to t+2**	**Δ*ROA***	**Δ*Returns***
Intercept	−0.04	−0.02	0.03	0.10	−0.03	0.09
	(0.06)	(0.50)	(0.73)	(0.24)	(0.17)	(0.32)
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.21	0.20	0.51	0.55	0.20	0.51
p-value	0.01	0.01	0.01	0.01	0.01	0.01
n	1178	930	1176	930	1178	1176

Performance is measured using return on assets (*ROA*) and 12-month market-adjusted stock returns (*Returns*). Two-tailed probability values are reported in parentheses. All explanatory variables are measured at the beginning of the repurchase year unless otherwise indicated.

Variable Definitions:

ΔROA = change in operating earnings scaled by total assets;
ΔReturns = change in 12-month stock returns ending on the fiscal year-end less the equally weighted return on the FTSE All Share index over the corresponding period;
Repurchase = indicator variable equal to 1 for repurchasing firms, and 0 otherwise;
EPS = indicator variable taking the value of 1 for firms with at least one of the *j* compensation components (*j* = bonus plans, option plans, or LTIPs) tied to EPS performance, and 0 otherwise;
$\mathit{Repurchase}_{cash\ rich}$ = indicator variable taking the value of 1 for repurchasing firms where either *Surplus cash holdings, Free cash flow*, or *Excess investing cash flow* from Table 3 is equal to 1 at time t−1, and 0 otherwise;
$\mathit{Repurchase}_{Cash\ constrained}$ = indicator variable taking the value of 1 for repurchasing firms where *Surplus cash holdings, Free cash flow*, and *Excess investing cash flow* from Table 3 are equal to 0 at time t−1, and 0 otherwise;
$\mathit{Nonrepurchase}_{Cash\ rich}$ = indicator variable taking the value of 1 for nonrepurchasing firms where either *Surplus cash holdings, Free cash flow*, or *Excess investing cash flow* from Table 3 is equal to 1 at time t−1, and 0 otherwise;
ROA = return on assets;
Negative earnings = indicator variable taking the value of 1 for firms reporting a loss, and 0 otherwise;
Returns = 12-month market-adjusted stock returns;
Log(total assets) = natural logarithm of total assets;
ΔLog(total assets) = change in the natural logarithm of total assets over the same period as the dependent variable is measured;
Market-to-book = book value of debt plus the market value of equity divided by total assets;
Leverage = total liabilities divided by total assets;
Working capital = change in noncash current assets minus liabilities scaled by total assets;
Investing cash flow = cash flow from investing activities scaled by total assets;
Operating cash flow = cash flow from operating activities scaled by total assets;
Cash holdings = cash and cash equivalents scaled by total assets;
Options outstanding = aggregate number of outstanding options for all employees scaled by market capitalization;
Industry effects = vector of industry indicator variables based on Datastream level-3 classification; and
Year effects = vector of calendar year indicator variables.

ers and firms with EPS-based performance conditions, respectively, the associated two-way interaction effect, and a set of controls. Columns 2 and 3 (4 and 5) in Table 4 report results for Δ*ROA* (Δ*Returns*). Findings provide no evidence that repurchasers in general, or repurchases with EPS conditions in particular, exhibit inferior post-repurchase performance. Estimated coefficients on the *Repurchase* indicator variable are either insignificant in the Δ*ROA* models or positive in the

ΔReturns models, while coefficient estimates on *Repurchase* × *EPS* are consistently insignificant. Our findings do not support the view that EPS-based performance targets encourage management to divert cash from positive NPV projects to fund buybacks.

The incentive to channel funds away from profitable investments to support repurchases may be especially strong among the subset of firms with limited cash resources. Columns 6 and 7 in Table 4 extend the previous analysis by replacing *Repurchase* with the following three indicator variables to capture firms' pre-repurchase cash characteristics: $Repurchase_{Cash\ rich}$ takes the value of 1 for repurchasing firms where either *Surplus cash holdings, Free cash flow*, or *Excess investing cash flow* from Table 3 equals 1 at time $t-1$, and 0 otherwise; $Repurchase_{Cash\ constrained}$ takes the value of 1 for repurchasing firms where *Surplus cash holdings, Free cash flow*, and *Excess investing cash flow* equal 0 at time $t-1$, and 0 otherwise; and $Nonrepurchase_{Cash\ rich}$ takes the value of 1 for nonrepurchasing firms where either *Surplus cash holdings, Free cash flow*, or *Excess investing cash flow* equals 1 at time $t-1$, and 0 otherwise. All three indicators are also interacted with *EPS* to capture incremental effects associated with EPS-based compensation. Absent EPS targets, cash-constrained repurchasers are associated with similar accounting performance (column 6) and superior market performance (column 7) in the post-repurchase period relative to other groups. Coefficient estimates on the $Repurchase_{Cash\ constrained}$ × *EPS* interaction are either positive (*ΔROA* model) or indistinguishable from 0 (*ΔReturn* model). Collectively, these findings provide no evidence that cash-constrained repurchasers, as a group, are characterized by abnormally weak future performance, or that the subset of cash-constrained repurchasers with EPS conditions exhibit incrementally worse performance.

Profitable investment opportunities are not the only source of funds for repurchases that could impose costs on shareholders. A second potentially costly source is regular dividend payments. Because dividends are sticky and dividend cuts are associated with significant stock price declines, regular dividend payments represent a permanent commitment to distribute cash flows and, as such, serve as a disciplinary force on managers' actions (Easterbrook 1984). Repurchases, by comparison, do not imply the same ongoing commitment and are therefore more suited to distributing transitory cash flow shocks (Guay and Harford 2000; Jagannathan et al. 2000). Using cash flows underpinning dividends to fund repurchases could therefore weaken monitoring through a lower commitment to ongoing payouts. Alternatively, repurchases may be associated with increased payouts to shareholders if they enable managers to disgorge lumpy cash surpluses in a timely fashion alongside regular dividend payments.

We examine the impact of EPS-motivated repurchases on payouts to shareholders in two ways. First, we model the annual change in total payouts (dividends plus repurchases scaled by lagged total assets) in the repurchase year using a difference-in-differences tobit regression. Explanatory variables include indicator variables for repurchasers and firms with EPS-based performance conditions, the associated two-way interaction effect, and a vector of controls. Column 2 in Table 5 presents results estimated using the full sample. The *Repurchase* coefficient estimate is positive and significant, indicating that repurchasers experience higher payout increases in the repurchase year. The estimated coefficient on *Repurchase* × *EPS* is not significant at conventional levels; payout increases for repurchases with EPS-contingent compensation are indistinguishable from those exhibited by non-EPS repurchasers. These results do not support dividend substitution claims for repurchasers in general and EPS-contingent repurchasers in particular. Instead, findings suggest that dividends and repurchases represent complementary payout options that in conjunction yield higher payouts to shareholders regardless of whether repurchases are driven by EPS-contingent compensation arrangements.

Columns 3 and 4 in Table 5 examine change in total payouts for firms with and without surplus cash at time $t-1$, respectively. Results and conclusions are similar to those reported for the full sample in column 2. While results in column 3 provide little evidence that EPS targets are

TABLE 5

Coefficient Estimates and Model Summary Statistics for Pooled Regressions Relating Payout Policy to Repurchase Activity and the Incidence of EPS-Based Performance Conditions in Executive Compensation Contracts

	Tobit Models: Δ*Total Payout*			OLS Model
Variable	**Full Sample**	**Surplus Cash: Yes**	**Surplus Cash: No**	***Dividend Forecast Error***
Surplus cash holdings	0.01			0.03
	(0.06)			(0.24)
Free cash flow	−0.01			0.06
	(0.10)			(0.02)
Excess investing cash flow	−0.01			−0.06
	(0.05)			(0.04)
Log(Market capitalization)	0.00	0.00	0.00	−0.01
	(0.09)	(0.19)	(0.37)	(0.23)
Market-to-book	0.00	0.01	−0.00	0.00
	(0.20)	(0.01)	(0.57)	(0.92)
Net leverage	−0.01	−0.00	−0.01	−0.01
	(0.01)	(0.10)	(0.01)	(0.49)
Negative returns	−0.00	−0.00	0.01	−0.03
	(0.92)	(0.49)	(0.07)	(0.16)
Negative earnings	−0.01	−0.01	−0.03	−0.00
	(0.05)	(0.07)	(0.03)	(0.91)
Options outstanding	0.00	−0.00	0.00	−0.01
	(0.99)	(0.88)	(0.54)	(0.01)
Dividend yield	0.02	−0.00	0.02	0.02
	(0.51)	(0.96)	(0.49)	(0.89)
ROA	0.06	0.06	0.06	−0.03
	(0.01)	(0.01)	(0.08)	(0.66)
Δ*ROA*	−0.03	−0.02	−0.06	−0.08
	(0.05)	(0.07)	(0.17)	(0.22)
Repurchase	0.05	0.05	0.04	−0.04
	(0.01)	(0.01)	(0.01)	(0.18)
Repurchase × EPS	−0.01	−0.01	−0.01	0.06
	(0.30)	(0.36)	(0.72)	(0.10)
Intercept	−0.01	−0.01	−0.02	0.17
	(0.91)	(0.74)	(0.32)	(0.44)
Industry effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Likelihood ratio	1370.16	1088.99	301.37	
p-value	0.01	0.01	0.01	0.01
Adjusted R^2				0.04
n	1313	1053	260	1312
n left-censored	214	161	53	

Two-tailed probability values are reported in parentheses.

All explanatory variables are measured at the beginning of the repurchase year unless otherwise indicated.

(continued on next page)

TABLE 5 (continued)

Variable Definitions:
Δ*Total Payout* = change in the aggregate value of dividends plus stock repurchases (scaled by lagged total assets) over the period $t-1$ to t; tobit regression models for Δ*Total Payout* are estimated using three samples: all firms with available data (Full sample), observations where either ***Surplus cash holdings, Free cash flow***, or ***Excess investing cash flow*** from Table 3 are equal to 1 at time $t-1$ (Surplus Cash: Yes), and observations where ***Surplus cash holdings, Free cash flow***, and ***Excess investing cash flow*** from Table 3 are equal to 0 at time $t-1$ (Surplus Cash: No); and
Dividend Forecast Error = unexpected dividend changes (scaled by lagged market capitalization) over the period $t-1$ to t, computed using a portfolio-based application of Lintner's (1956) model;

Explanatory Variable Definitions:
Repurchase = indicator variable equal to 1 for repurchasing firms, and 0 otherwise;
EPS = indicator variable taking the value of 1 for firms with at least one of the j compensation components (j = bonus plans, option plans, or LTIPs) tied to EPS performance, and 0 otherwise;
Δ*ROA* = change in operating earnings scaled by total assets;
Industry effects = vector of industry indicator variables based on Datastream level-3 classification; and
Year effects = vector of calendar year indicator variables.

See Tables 2 and 3 for definitions of remaining variables.

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associated with significantly larger payout increases for surplus cash flow repurchasers, there is no suggestion of dividend substitution among low cash flow repurchasers with EPS-contingent compensation (column 4).

Our second test of whether EPS targets encourage managers to divert funds from regular dividend increases to pay for repurchases focuses on dividend forecast errors, defined as the observed dividend change minus the expected dividend change. If repurchasers redirect funds from dividend payments to support repurchase activity, then we should observe lower-than-expected dividend increases in the repurchase year. We draw on Lintner's (1956) model linking current dividends to contemporaneous earnings and lagged dividend payments to estimate expected dividend changes for firm i in year t. Because we are unable to estimate firm-specific regression parameters from Lintner's (1956) model due to insufficient time-series data for earnings and dividends, we instead use a portfolio approach based on the following sequential sort procedure. Each year all nonfinancial firms from Extel with available data are sorted into quintiles based on contemporaneous earnings before exceptional items. Each annual earnings portfolio is then further sorted into quintile portfolios according to lagged ordinary dividends. Median annual dividend changes for the resulting 25 earnings-lagged dividend portfolio combinations (computed after excluding repurchasers) serve as estimates of expected dividend changes. Dividend forecast errors are equal to the observed annual dividend change less the median dividend change for firms' corresponding earnings-lagged dividend portfolio in year t. Column 5 in Table 5 reports coefficient estimates from an OLS regression of dividend forecast errors on the *Repurchase* indicator variable, its corresponding interaction with *EPS*, and a vector of control variables. Although the estimated coefficient on *Repurchase* is negative, as predicted by the dividend-substitution hypothesis, it is not significant at conventional levels ($p = 0.18$). Further, the incremental effect for repurchasers with EPS targets (*Repurchase* × *EPS*) is positive and marginally significant (p = 0.10). Overall, results do not support the view that EPS-contingent compensation arrangements motivate firms to divert funds from ordinary dividend payments to support repurchases.[10]

Efficient Contracting

Linking executive compensation to EPS growth provides management with a means of manipulating reported performance (through repurchases) that can easily be avoided by using alternative accounting metrics such as ROA or growth in operating profit. Why EPS conditions remain popular in executive compensation contracts despite the additional earnings management opportunities such arrangements provide is, therefore, an intriguing question. One possibility is that EPS conditions represent an efficient contracting device that helps align interests of managers and shareholders by incentivizing managers to make decisions that promote shareholder value. Prior research highlights several shareholder benefits associated with repurchases including limiting overinvestment of surplus cash, increasing leverage in firms with inefficient capital structure, and signaling stock price undervaluation (Dittmar 2000). Insofar as EPS targets create incentives to manipulate reported performance by repurchasing stock, EPS-based compensation may provide shareholders with a simple means of motivating executives to distribute surplus cash, increase leverage, and correct underpricing in a timely manner.

Table 6 presents models testing whether the positive association between repurchases and cash is more pronounced for firms with EPS performance conditions in the presence of surplus cash. We create an indicator variable, *Surplus cash,* taking the value of 1 where *Surplus cash holdings, Free cash flow,* or *Excess investing cash flow* from Table 3 is equal to 1, and 0 otherwise. We use *Surplus cash* to partition the sample before estimating separately for each partition a logistic (left-censored tobit) regression relating the probability (scaled value) of a repurchase to stock and flow measures of cash and their associated interactions with EPS. Estimated coefficients on the *Cash holdings* × *EPS, CFO* × *EPS,* and *CFI* × *EPS* interactions are positive and significant in columns 3 and 4, where *Surplus cash* is equal to 1 (except for *Cash holdings* × *EPS* in the logistic model). In contrast, only the *CFO* × *EPS* coefficient (tobit model) is positive and significant in columns 5 and 6, where *Surplus cash* is equal to 0. On balance, these findings suggest that EPS targets benefit shareholders by creating a stronger link between repurchases and cash performance in the presence of surplus cash.

Table 7 examines the intervening effect of low leverage and undervaluation on the link between repurchases and EPS targets. In Models 1 and 2, we estimate a version of Equation (1) relating the probability of a repurchase to the presence of EPS targets and an underleverage indicator variable. *Underleverage* takes the value of 1 when net leverage is less than the annual median value for the corresponding Datastream level-4 industry group (computed using the Extel population), and 0 otherwise. Model 1 includes *Underleverage* as a main effect, while Model 2 includes the *Underleverage* × *EPS* interaction. As predicted, estimated coefficients for the *Underleverage* main effect are positive and significant in both models, as are the coefficients on *EPS.* The *Underleverage* × *EPS* variable in Model 2, however, does not load (p = 0.93). Accordingly,

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[10] In supplementary tests we examined off-diagonal cases based on the predicted probabilities from a pooled version of Model 1 in Table 3. We classified repurchasers with an implied probability ≤0.5 as "unexpected repurchasers" and nonrepurchasers with an implied probability >0.5 as "unexpected nonrepurchasers." We compared these cases with observations where the model correctly predicts a repurchase (firm is a repurchaser and the implied probability > 0.5) and a nonrepurchase (firm is a nonrepurchaser and the implied probability ≤0.5). We find no evidence that unexpected repurchasers perform worse or make lower payouts than expected repurchasers. These results provide further evidence that EPS-based compensation arrangements do not encourage firms to engage in inconsistently suboptimal repurchase activity. On the other hand, there is some suggestion that unexpected nonrepurchasers perform worse than expected nonrepurchasers, consistent with the view that failure to buy back shares in particular situations imposes agency costs on shareholders (e.g., through overinvestment).

TABLE 6

Coefficient Estimates and Model Summary Statistics for Pooled Logistic (Left-Censored Tobit) Regressions Relating the Probability (Scaled Value) of a Repurchase to the Incidence of EPS-Based Performance Conditions in Executive Compensation Contracts, Conditional on Surplus Cash

		Surplus Cash: Yes		**Surplus Cash: No**	
Variable	**Predicted Sign**	**Logistic**	**Tobit**	**Logistic**	**Tobit**
Cash holdings	(+)	−3.63	−0.11	0.30	−0.02
		(0.01)	(0.01)	(0.68)	(0.54)
CFO	(+)	3.63	0.14	6.88	0.18
		(0.01)	(0.01)	(0.03)	(0.01)
CFI	(+)	1.34	0.03	6.20	0.14
		(0.01)	(0.04)	(0.09)	(0.08)
Log(Market capitalization)	(?)	0.07	0.00	0.52	0.01
		(0.12)	(0.10)	(0.01)	(0.01)
Market-to-book	(−)	−0.18	0.01	−0.68	−0.01
		(0.10)	(0.01)	(0.03)	(0.48)
Net leverage	(−)	−1.57	−0.02	−0.38	−0.02
		(0.01)	(0.01)	(0.01)	(0.01)
Dividend yield	(?)	−0.67	−0.04	29.80	−0.03
		(0.53)	(0.44)	(0.01)	(0.52)
Negative returns	(+)	−0.05	−0.00	0.40	0.02
		(0.75)	(0.89)	(0.31)	(0.18)
Negative earnings	(−)	−1.14	−0.04	−1.13	−0.02
		(0.01)	(0.01)	(0.12)	(0.32)
Options outstanding	(+)	−0.02	−0.00	0.02	0.01
		(0.06)	(0.35)	(0.77)	(0.47)
NDISC	(?)	1.23	0.03	2.27	0.07
		(0.01)	(0.01)	(0.02)	(0.01)
EPS	(+)	0.45	0.01	−0.35	0.00
		(0.09)	(0.91)	(0.65)	(0.99)
EPS × Cash holdings	(+)	0.50	0.07	−1.39	−0.19
		(0.60)	(0.05)	(0.63)	(0.01)
EPS × CFO	(+)	2.55	0.10	6.00	0.19
		(0.06)	(0.05)	(0.13)	(0.05)
EPS × CFI	(+)	3.42	0.15	1.33	0.04
		(0.01)	(0.01)	(0.76)	(0.71)
Intercept	(?)	−0.02	−0.04	−4.09	−0.09
		(0.96)	(0.01)	(0.01)	(0.01)
Industry		Yes	Yes	Yes	Yes
Year		Yes	Yes	Yes	Yes
Likelihood ratio		233.10	335.66	145.47	90.49
n repurchasers/left-censored		553	516	112	149
Pseudo R^2		0.26		0.57	
% classified correctly		64.50		69.70	
n		1069	1069	261	261

Two tailed probability values are reported in parentheses.

(continued on next page)

TABLE 6 (continued)

The sample is partitioned according to the presence or absence of surplus cash. Columns headed "Surplus Cash: Yes" contain observations where either the *Surplus cash holdings*, *Free cash flow*, or *Excess investing cash flow* indicator variables from Table 3 equals 1. Columns headed "Surplus Cash: No" contain observations where the *Surplus cash holdings*, *Free cash flow*, and *Excess investing cash flow* indicator variables from Table 3 are equal to 0. In columns headed "Logistic" the dependent variable is the logarithm of the odds of a repurchase. In the columns headed "Tobit" the dependent variable is the aggregate amount (including expenses) spent repurchasing shares during fiscal year *t*, scaled by lagged total assets.

All variables are measured at the beginning of the repurchase year.

Explanatory Variable Definitions:

NDISC = indicator variable taking the value of 1 when insufficient disclosure renders the presence of an EPS target indeterminate, and 0 otherwise;
EPS = indicator variable taking the value of 1 for firms with at least one of the *j* compensation components (*j* = bonuses, options, or LTIPs) linked to EPS performance, and 0 otherwise;
Cash holdings = cash and cash equivalents scaled by lagged total assets;
CFO = cash flow from operations scaled by lagged total assets;
CFI = cash flow from investing activities scaled by lagged total assets;
Market capitalization = share price multiplied by the number of shares outstanding;
Market-to-book = book value of debt plus the market value of equity divided by total assets;
Net leverage = total liabilities net of cash holdings divided by total assets net of cash holdings;
Dividend yield = ordinary dividends per share divided by share price;
Negative returns = indicator variable taking the value of 1 if 12-month stock returns are less than the market return over the corresponding period, and 0 otherwise;
Negative earnings = indicator variable taking the value of 1 if reported EPS is negative, and 0 otherwise; and
Options outstanding = aggregate number of outstanding options for all employees at the balance sheet date scaled by market capitalization.

while findings support claims that repurchases are a response to abnormally low leverage, there is no evidence that EPS-based compensation arrangements amplify this effect.

Models 3 and 4 in Table 7 explore the link between equity undervaluation and EPS-motivated repurchases. Our proxy for undervaluation compares observed price with intrinsic value estimated using the harmonic mean of price-to-forward earnings (Liu et al. 2002):

$$IV_{it-1} = E^{F1}_{it-1} \times \frac{1}{\frac{1}{n}\sum_{j=1}^{J}\frac{E^{F1}_{jt-1}}{P_{jt-1}}}, \tag{3}$$

where IV is intrinsic value for firm i four months after the beginning of repurchase year t, E^{F1} is the last available I/B/E/S mean consensus forecast for one-year-ahead EPS during the window -300 days $< t <$ 120 days, P is observed stock price four months after the end of fiscal year t -1, J is the set of firms with consensus forecast data on I/B/E/S in the same Datastream level-3 industry as firm i ($i \notin J$), and n is the number of firms in J. The indicator variable *Undervaluation* takes the value of 1 where $IV_{it-1} > P_{it-1}$, and 0 otherwise. Model 3 in Table 7 includes the *Undervaluation* main effect, while Model 4 is expanded to include the *Undervaluation* × *EPS* term. As with all previous models, *EPS* loads with a statistically significant positive coefficient in both regressions. The *Undervaluation* main effect also loads positively in Model 3, suggesting that repurchases are at least a partial response to market mispricing. However, Model 4 suggests this effect is driven by firms with EPS-based compensation conditions: the coefficient on *Undervaluation* is indistinguishable from 0, whereas *Undervaluation* × *EPS* is positive and significant at the 10 percent level. (The probability value for a likelihood ratio test comparing Models 3 and 4 is 0.09.) An indication of the economic significance of the interaction effect is provided by compar-

TABLE 7

Coefficient Estimates and Model Summary Statistics for Conditional Logistic Regressions Examining the Intervening Effect of Underleverage and Undervaluation on the Association between the Probability of a Repurchase and the Incidence of EPS-Based Performance Conditions in Executive Compensation Contracts

Variables	**Predicted Sign**	**Underleverage**		**Undervaluation**	
		Model 1	**Model 2**	**Model 3**	**Model 4**
Log(Market capitalization)	(?)	1.21	1.21	1.56	1.56
		3.34	*3.35*	*4.74*	*4.76*
		(0.01)	(0.01)	(0.01)	(0.01)
Market-to-book	(−)	−0.67	−0.68	−0.67	−0.67
		0.51	*0.51*	*0.51*	*0.51*
		(0.01)	(0.01)	(0.01)	(0.01)
Net leverage	(−)			−0.37	−0.41
				0.69	*0.66*
				(0.10)	(0.07)
Dividend yield	(?)	0.90	0.90	0.65	0.65
		2.45	*2.45*	*1.92*	*1.91*
		(0.33)	(0.33)	(0.57)	(0.57)
Negative returns	(+)	−0.04	−0.04		
		0.96	*0.96*		
		(0.78)	(0.78)		
Negative earnings	(−)	−1.08	−1.08	−1.26	−1.23
		0.34	*0.34*	*0.28*	*0.29*
		(0.01)	(0.01)	(0.01)	(0.01)
Options outstanding	(+)	−0.02	−0.02	0.01	0.01
		(0.98)	*(0.98)*	*(1.01)*	*(1.01)*
		(0.12)	(0.12)	(0.75)	(0.71)
Surplus cash holdings	(+)	0.15	0.15	−0.24	−0.25
		1.16	*1.16*	*0.78*	*0.78*
		(0.33)	(0.33)	(0.22)	(0.22)
Free cash flow	(+)	0.63	0.63	0.52	0.53
		1.87	*1.87*	*1.67*	*1.69*
		(0.01)	(0.01)	(0.03)	(0.03)
Excess investing cash flow	(+)	0.72	0.72	0.83	0.83
		2.06	*2.06*	*2.30*	*2.29*
		(0.01)	(0.01)	(0.01)	(0.01)
Underleverage	(+)	0.42	0.40		
		1.52	*1.49*		
		(0.01)	(0.12)		
Undervaluation	(+)			0.76	0.10
				2.13	*1.11*
				(0.01)	(0.83)
NDISC	(?)	1.46	1.46	1.70	1.85
		4.33	*4.33*	*5.46*	*6.36*
		(0.01)	(0.01)	(0.01)	(0.01)
EPS	(+)	0.67	0.65	0.95	0.68
		1.95	*1.92*	*2.57*	*1.98*
		(0.01)	(0.01)	(0.01)	(0.03)

(continued on next page)

TABLE 7 (continued)

Variables	Predicted Sign	Underleverage		Undervaluation	
		Model 1	**Model 2**	**Model 3**	**Model 4**
EPS × *Underleverage*	(+)		0.03 *1.03* (0.93)		
EPS × *Undervaluation*	(+)				0.79 *2.20* (0.10)
Likelihood ratio		267.44	268.17	207.81	210.56
p-value		0.01	0.01	0.01	0.01
Pseudo R^2		0.44	0.44	0.50	0.51
% correctly classified		73.50	73.70	78.10	78.80
n		665	665	439	439

Three values are reported for each covariate: the first value is the coefficient estimate; the second (italicized) value is the odds ratio; and the third value (in parentheses) is the two-tailed probability value.

The dependent variable is the logarithm of the odds of a repurchase. See Tables 2 and 3 for definitions of all remaining variables. All variables are measured at the beginning of the repurchase year.

Variable Definitions:

NDISC = indicator variable taking the value of 1 when insufficient disclosure renders the presence of an EPS target indeterminate, and 0 otherwise;

EPS = indicator variable taking the value of 1 for firms with at least one of the *j* compensation components (*j* = bonus plans, option plans, or LTIPs) tied to EPS performance, and 0 otherwise;

Underleverage = indicator variable taking the value of 1 where net leverage is less than the annual median value for the corresponding Datastream level-4 industry group, and 0 otherwise; and

Undervaluation = indicator variable taking the value of 1 where intrinsic value computed using a price-to-forward-earnings multiple is greater than observed price on the valuation date (four months after the beginning of the repurchase year), and 0 otherwise.

ing the odds ratios for *Undervaluation* in the presence and absence of EPS-contingent contracts.[11] The odds of repurchasing are no different than the odds of not repurchasing for undervalued firms in the absence of EPS-contingent compensation ($e^{0.1} = 1.11$). In contrast, the odds of a repurchase are $e^{0.1+0.79} = 2.44$ times higher than the odds of not repurchasing for undervalued firms with EPS-contingent compensation. Results provide further (albeit statistically weak) evidence that shareholders benefit from EPS-motivated repurchases in the form of higher buyback probability when stock price falls below intrinsic value.

Our final set of tests examines the interaction between stock repurchases and accrual management. Executives need not resort to repurchases to meet binding EPS targets. Prior research indicates that managers often use accounting accruals to maximize compensation payouts (Healy 1985). Although the choice between alternative earnings management instruments has not been widely explored in the literature, several factors suggest that U.K. shareholders favor EPS management through repurchases over accruals. First, because shareholders benefit from repurchases

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[11] Direct interpretation of coefficient estimates and odds ratios reported for interaction terms is problematic in nonlinear models since the effect depends on the contribution of the covariates (Ai and Norton 2003). Comparing odds ratios for the *j*th covariate in the presence and absence of the *i*th covariate provides a simple means of interpreting the economic significance of the *j* × *i* interaction.

(see above), the net impact on shareholder wealth from managing EPS through this method is likely to be less detrimental than accruals, where the gains to shareholders are less obvious. Second, the effect of repurchases on reported EPS is more transparent than accrual choices because U.K. firms must disclose details of all repurchase trades to the stock market without delay and report aggregate repurchase activity in their published financial statements. Investors are therefore better placed to reverse the effect of repurchases on reported EPS, should they wish to do so. Third, unlike accruals that reverse over time, buybacks raise the baseline EPS target permanently and do not contribute to future earnings variability.[12]

If managing EPS through repurchases imposes lower net costs on shareholders relative to accrual manipulation, owners would benefit from compensation-driven buybacks via lower accrual manipulation as executives substitute repurchases for discretionary accruals. We test this prediction by regressing measures of absolute abnormal working capital accruals for the repurchase year on an indicator variable for repurchasers with EPS targets (*Repurchase_EPS*), the two-way interaction capturing the incremental effect for repurchasers without EPS conditions (*Repurchase_EPS* × *NOEPS*), and a vector of controls. We use the absolute value of abnormal accruals because our prediction relates to the overall level of accrual management activity.[13]

Abnormal working capital accruals are estimated using two methods: the modified-Jones model (Dechow et al. 1995) and the performance-matched model (Kothari et al. 2005). We estimate both models cross-sectionally using industry-year portfolios comprising all observations from Extel with available data. Results in Table 8 are consistent with lower abnormal accrual activity for repurchasers with EPS-contingent compensation arrangements, relative to nonrepurchasers and repurchasers without EPS targets. The estimated coefficient on *Repurchase_EPS* is negative and significant in all models, while the *Repurchase_EPS* × *NOEPS* coefficient is reliably positive and significant. These findings are consistent with the view that firms with EPS targets are more likely to manipulate reported EPS through repurchases as opposed to working capital accruals. To the extent that manipulation via accruals imposes higher costs on shareholders, these results suggest an additional channel through which the positive association between repurchases and EPS-contingent compensation can benefit shareholders.

Summary

Results presented in this section suggest net benefits to shareholders from stock repurchases motivated by EPS targets in executive compensation contracts. Repurchasers are associated with larger increases in total payouts, and this effect is no less pronounced for repurchasers with EPS targets, suggesting that dividend substitution is not a first-order concern. Further, the positive link between repurchases and cash performance is more prominent for firms with EPS targets in the presence of surplus cash; undervalued firms with EPS conditions are more likely to signal their

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[12] Managers may also favor repurchases over accruals as a means of inflating EPS for several reasons. First, because stock market investors typically view repurchases favorably, they are less likely to question executives' underlying repurchases motives. Second, executives can provide convincing, non-manipulation-based explanations to support their actions if challenged by shareholders.

[13] Using a levels approach to model absolute abnormal accruals introduces the risk of spurious correlation. Our results and conclusions should be interpreted with this caveat in mind. Several factors militate against using a changes specification however. First, accrual reversals confound a changes specification: absolute abnormal accrual levels may remain similar over short intervals even when the level of accrual management declines (because high absolute accruals capture both contemporaneous accrual management and unwinding of accrual management from previous periods). Second, if firms use repurchases instead of accruals in response to a new stimulus for EPS manipulation, then absolute accruals will remain constant over time and a changes specification will yield null results. Consistent with these arguments, when we model the change in absolute accruals we find no difference between repurchasers with EPS conditions, repurchasers without EPS conditions, and nonrepurchasers.

TABLE 8

Coefficient Estimates and Model Summary Statistics from Pooled OLS Regressions Relating Absolute Abnormal Accrual Activity to Repurchase Activity and a Vector of Control Variables

		Absolute Abnormal Accruals Computed Using:			
		DSS Model		KLW Model	
Variable	**Predicted Sign**	**Model 1**	**Model 2**	**Model 1**	**Model 2**
Log(Total assets)	(?)		0.00		0.00
			(0.19)		(0.04)
Market-to-book	(?)		0.00		0.00
			(0.29)		(0.14)
Net leverage	(+)		0.01		0.01
			(0.01)		(0.01)
Negative earnings	(+)		0.01		0.01
			(0.14)		(0.52)
Options outstanding	(+)		0.00		0.00
			(0.24)		(0.11)
σOperating cash flow	(+)		0.18		0.20
			(0.01)		(0.01)
Lagged absolute accruals	(+)		0.05		0.05
			(0.01)		(0.01)
Repurchase_EPS	(−)	−0.02	−0.02	−0.02	−0.01
		(0.01)	(0.02)	(0.01)	(0.04)
Repurchase_EPS × NOEPS	(+)	0.02	0.02	0.02	0.02
		(0.05)	(0.05)	(0.02)	(0.05)
Intercept	(?)	0.10	0.09	0.10	0.09
		(0.01)	(0.01)	(0.01)	(0.01)
Industry		No	Yes	No	Yes
Year		No	Yes	No	Yes
Adjusted-R^2		0.01	0.09	0.01	0.07
p-value		0.01	0.01	0.01	0.01
n		1314	1312	1314	1312
n repurchasers		656	654	656	654

Two-tailed probability values are reported in parentheses. The dependent variable is the absolute value of abnormal working capital accruals, where abnormal accruals are the residual from either the Dechow et al. (1995) working capital accrual model (DDS) or the Kothari et al. (2005) working capital accrual model (KLW). Both accrual models are estimated cross-sectionally for all industry-year combinations.

All variables relate to repurchase year *t*.

Variable Definitions:

Repurchase_EPS = indicator variable equal to 1 for repurchasing firms with at least one of the *j* compensation components (*j* = bonus plans, option plans, or LTIPs) tied to EPS performance, and 0 otherwise;

NOEPS = indicator variable taking the value of 1 for firms that do not have at least one of the *j* compensation components tied to EPS performance, and 0 otherwise;

Total assets = balance sheet value of aggregate assets;

Market-to-book = book value of debt plus the market value of equity divided by total assets;

Net leverage = total liabilities net of cash holdings divided by total assets net of cash holdings;

(continued on next page)

TABLE 8 (continued)

Negative earnings = indicator variable taking the value of 1 if reported earnings per share are negative, and 0 otherwise;
Options outstanding = aggregate number of outstanding options for all employees at the balance sheet date scaled by market capitalization;
σ*Operating Cash flow* = standard deviation of operating cash flow scaled by lagged total assets computed over the three-year period centered on year t;
Lagged accruals = one-year lagged value of the dependent variable;
Industry = vector of industry indicator variables based on Datastream level-3 classification; and
Year = vector of calendar year indicator variables.

undervaluation through a repurchase; and repurchasers with EPS conditions are associated with lower abnormal accruals. We find no evidence that EPS-driven repurchases impose costs on shareholders in the form of investment myopia.

VI. CONCLUSIONS

This study examines the impact on firms' stock repurchase activity of EPS performance conditions in executive compensation contracts. Our analysis connects three distinct literatures. One body of research demonstrates how aspects of corporate payout policy are sensitive to executives' compensation arrangements. Another body of work based on surveys and anecdotal evidence indicates that managers are sensitive to the EPS impact of repurchases. A third group of studies concludes that managers use repurchases to achieve key EPS performance thresholds. Our analysis integrates these three literatures by examining how repurchase policy is shaped by contractual arrangements that create an explicit link between executive compensation and reported EPS.

We find that EPS targets explain firm-level repurchase policy after controlling for traditional determinants of buybacks. Further analysis reveals that EPS-motivated repurchases yield net benefits to shareholders. Contrary to Bens et al. (2002), we find no evidence that EPS-driven repurchases lead to investment myopia. Instead, repurchasers are associated with larger payout increases to shareholders that partly reflect a more pronounced link between repurchases and cash performance for firms with EPS targets in the presence of surplus cash flow. In addition, undervalued firms with EPS targets are more likely to signal mispricing through a repurchase, and repurchasers with EPS conditions are associated with lower abnormal accrual activity.

With repurchases emerging as a key payout mechanism in many jurisdictions and in view of concerns about the motives underlying this trend (Bens et al. 2002; Bens et al. 2003; Hribar et al. 2006; Marquardt et al. 2009), a better understanding of the contractual incentives driving repurchase activity and their associated economic consequences is appropriate. Our findings reveal significant contracting benefits from the repurchase incentives that result from linking executive compensation to EPS. In particular, we identify stock repurchases as a potentially important benefit of EPS-based targets in executive compensation contracts that improves alignment of managers' and shareholders' interests. This insight is consistent with Huang et al. (2010) who find that EPS-based bonus plans help to address agency conflicts between managers and shareholders in the form of ownership dilution. We therefore provide a modest further step toward a more complete understanding of the costs and benefits associated with per-share-based performance measures in general, and in particular why EPS-based targets remain a popular choice in executive compensation contracts despite their obvious limitations.

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BOOK REVIEWS

Stephen A. Zeff, Editor

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DAVID ABOODY and RON KASZNIK, *Executive Compensation and Financial Accounting*, Foundations and Trends® in Accounting (Hanover, MA: now Publishers, Inc., 2009, ISBN: 978-1-60198-342-8, Vol. 4, No. 2, pp. xi, 88).

David Aboody and Ron Kasznik's monograph on the relation between executive compensation and financial reporting explores the interplay of regulation and pay. They organize their review around two themes: (1) compensation provides incentives to influence financial reporting (Chapters 2 and 3) and (2) financial reporting and regulation, more broadly, influence the compensation contracting process (Chapters 4 and 5). The authors do a nice job explaining important institutional details, summarizing much of the relevant literature, and offering potential explanations for conflicting evidence.

The literature in this area is vast and cuts across several disciplines; so no monograph could include all relevant research. And compensation practices and regulations are ever evolving; so new questions continue to arise. Besides highlighting the key features of Aboody and Kasznik's monograph, my aim in this review is to introduce the reader to work that provides different perspectives and highlight additional areas for future research.

I. EFFECTS OF COMPENSATION ON FINANCIAL REPORTING CHOICE

With apologies to the Rolling Stones, this section of the monograph could be subtitled "Maybe you can't always get what you want, but, with a rational economic agent, you just might find you get what you reward." In this section, the authors describe the literature, examining the incentive effects of compensation contracts on managers' financial reporting choices.

In Chapter 2, they summarize the literature on the influence of earnings-based contracts (i.e., bonuses) and stock option compensation on accounting choices. They provide a broad survey of the research in this area and highlight the limitations of studying discretionary accounting choices, including the inability to cleanly measure discretion.

The focus of Chapter 3 is the literature, examining the abnormal (v-shaped) stock price pattern around option grants and, in particular, the influence of stock option grants on voluntary disclosures. The authors primarily discuss their own work, Aboody and Kasznik (2000), which is appropriate, as they were the first researchers to try to disentangle whether managers were timing option grants around corporate disclosures, or vice versa.

The authors provide a survey of the key research in this area and leave the reader with directions for future research. One direction they do not mention is the incentives created by features of other types of equity grants. For example, in the U.K. and increasingly in the U.S., firms are granting stock and stock options with performance-vesting features. Performance requirements can create direct incentives to manipulate earnings or stock price, and reporting requirements make the performance targets more transparent to researchers. But other features of these contracts may alter those incentives (i.e., performance conditions measured over time or proportional vesting over a range of outcomes). Prior research has studied other aspects of these compensation contracts, but exploring the incentive effects of these other types of equity may be a fruitful direction for future work.[1]

The literature in this section of the monograph demonstrates that compensation contracts can encourage self-serving behavior. Thus, the authors frequently pose the question, "Why would shareholders allow these contracts?" In Section 2.3, they present formally the idea that perhaps creating incentives to make choices is *exactly* in the shareholders' interest and not necessarily rent extraction. They then describe one of their studies, Aboody and Kasznik (2008), as an illustration of contracts motivating what appears to be self-serving behavior but actually serving shareholders' interests. The literature proposes other answers to their question, and thus a broader perspective here would have been helpful. For example, one such answer might be that shareholders are still better off with these contracts, even though they encourage self-serving decisions. Evans and Sridhar (1996) offer insights on this point. Their work considers the trade-off between reporting truthfully and managing earnings, and it recognizes that the firm has two levers at its disposal: the compensation contract that rewards effort, and the financial reporting system that may or may not constrain earnings management. In their model, it can be optimal to offer a compensation contract that may result in earnings management because it would be too costly to eliminate it. Said differently, executives' having skin in the game still provides a greater benefit to shareholders even if, on the margin, executives extract rents.

II. EFFECTS OF REGULATION ON THE DESIGN OF COMPENSATION

The second half of the monograph examines other forces that shape executive compensation—namely, accounting and tax regulation. The authors first take on the question of whether the favorable (i.e., non-expensing) treatment of options prior to SFAS 123R influenced their use. In summarizing that literature, they describe studies that examine the underlying assumption that non-expensing would benefit the firm, possibly through the perception of greater profitability. They then review studies that examine the influence of tax regulation on the design of compensation, focusing on Section 162(m) and various tax treatments of stock options.

The authors provide a nice description of the institutional details of the related accounting and tax issues and draw from a variety of studies in accounting, finance, economics, and management. They also provide critical context for considering the mixed results of the early literature and for assessing the validity of the conclusions of the early literature when compared with later work.

Scholars interested in this area might also consider some additional work. Hodder et al. (2006), for example, provide evidence that discretion allowed in determining option valuation inputs does not necessarily result in Black-Scholes assumptions that are value-decreasing. This study provides an interesting contrast to the studies described in the monograph that suggest that managers use this discretion to lower option expense. Similarly, researchers doing experimental work provide additional evidence that might shape studies examining how the market values stock option expense. For example, Frederickson et al. (2006) provide evidence that financial statement users accord greater reliability to reported over-disclosed expense, while Libby et al. (2006) argue that auditors may require greater accuracy in recognized over-disclosed expense. These findings suggest that the market valuation of stock option compensation may differ under the new accounting treatment. Finally, many studies, too numerous to list, have questioned whether the market values stock options as an expense or as a net benefit. Though diverse in their conclusions, likely from the variety of research designs, these studies may help researchers drill deeper into that question.

As in the prior section of their monograph, the authors lay out many directions for future research, but they may undersell the potential for future work on the relation between tax regulation and pay. While the Tax Code Section 162(m) provides fodder for a stream of research in this area, other tax regulations related to compensation, such as Section 83(b) and Section 409A, may provide interesting additional "frictions." And, unlike the challenges in examining accounting discretion, these studies are able to exploit more "exogenous" events like tax rate changes.

III. GENERAL COMMENTS

The monograph underscores an important message in this line of research, namely, that compensation contracts are shaped by many forces: desire to align incentives, tax rules, financial reporting consequences, resource (cash) constraints, and political costs, among others. All of these forces occur in a labor market where talented executives may demand specific contractual features. This real-world complexity makes research challenging, but we, as scholars, should not be dissuaded by less-than-perfect research designs. A messy world will, at times, yield messy conclusions, but there still might be something we can learn.

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[1] See for example Gerakos et al. (2007), Bettis et al. (2008), and Carter et al. (2009).

I fear that the authors sometimes overlook this reality. In this vein, they are, perhaps, too critical of the backdating research. They note (on page 51) that "inferring backdating in large samples using only publicly available data is challenging. Specifically, it is nearly impossible for researchers ... to disentangle backdating from other opportunistic timing of option awards ... or opportunistic timing of corporate announcements." This criticism could be leveled at much of the research in this area. And, while undoubtedly imperfect, the results in Lie (2005) were convincing enough to have a hefty impact on regulators and practice.

Similarly, Aboody and Kasznik interpret the mixed results of studies that attempt to document a link between pay and performance as suggesting (on page 2) "weakness in the premise of incentive alignment as a principal determinant of executive compensation." Indeed, this is one of the most fundamental questions of this field, as one solution to the moral hazard problem is the "carrot": deliver better performance, and you shall be rewarded. I would offer, instead, that the mixed results reflect the difficulty of doing work in this area. As researchers, it is rare for us to observe the underlying contract; we only see the payoffs from the contracts. Those payoffs reflect both short-term and long-term objectives of the firm and may rely on performance measures different from accounting or stock price performance often used in these studies. Researchers often must make assumptions about the *ex ante* contract and, to the extent they are wrong, those assumptions weaken the power of their tests.

IV. WHAT NEXT?

At the end of Chapter 5, the authors highlight recent developments, including the Troubled Asset Relief Program, which potentially created frictions in contracting by constraining compensation for participating banks. They also mention the recently enacted "say on pay" legislation, which will increase the visibility of executive compensation. But there are other developments that dovetail with the theme of their monograph. The financial crisis, for example, has engendered real debate about the role that compensation played. Did performance-based pay encourage excessive risk-taking by some firms? Another development is changes to required proxy statement disclosure effective in 2006 and 2010. These new disclosures provide data that allow greater insight into the *ex ante* features of contracts, putting researchers in a better position to examine previously difficult-to-address questions with potentially stronger research designs.

Overall, Aboody and Kasznik offer a coherent and thought-provoking summary of the literature in this area and point to many possibilities for future research. With the continual evolution of compensation practices and related regulation, this is a literature with growth opportunities. To quote Mick Jagger, who, after all, attended the London School of Economics, "A good thing never ends."

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MARY ELLEN CARTER
Associate Professor of Accounting
Boston College

JAN PFISTER, *Managing Organizational Culture for Effective Internal Control: From Practice to Theory* (Berlin, Germany: Physica-Verlag, 2009, ISBN 978-3-7908-2339-2340-0, pp. xx, 245).

This book presents the design and outcomes of a field study that aims to develop and validate a new framework for internal control in organizations. It starts with a quick motivation of the importance of such an exercise, which points to the recent (near) collapses of financial institutions across the Western world. Arguing that such failures "can be linked to internal control and organizational culture" (p. 1), the author claims the need to understand the combination of effects of internal control and organizational culture on managerial behavior and organizational outcomes. As, these days, probably few would oppose such a need, the study appears to be a timely academic response to contemporary concerns about the effectiveness of organizational control systems, and potential detrimental effects of organizational culture.

The author's analysis aims to unravel how internal control and organizational culture interact to produce organizational outcomes, guided by an overarching research question that is formulated as follows: "How do management principles and practices affect organizational culture in a way so that organizational culture positively impacts internal control effectiveness?" (p. 6). Whereas the research question proposes a unidirectional model from "management principles and practices," via "organizational culture" to "internal control effectiveness," the author points out elsewhere that the two main challenges of the study lie in capturing "the influence of organizational culture on internal control effectiveness" (p. 3) and "how control mechanisms influence organizational culture" (p. 3), arguing that "not only does culture influence control, but control also influences culture" (p. 3). Disregarding the apparent ambiguity in terminology, the word "challenge" itself seems aptly chosen, because an analysis of the relationships between culture and control is no trivial exercise for theoretical and empirical as well as for conceptual reasons. Chapter 1 therefore starts by presenting definitions of internal control and organizational culture with the aim to delineate them for analytical purposes (pp. 1–2) and continues by pointing out five "broad issues" (p. 5) that the study aims to address. These "broad issues" are problems distilled from the extant literature in management accounting and control, and are presented as "open questions" on the relationship between control and culture that guide the analysis (pp. 5–6).

I find these different layers of proposing the study's structure to be confusing, as it takes some guessing to understand where the author wants our thoughts to go. For example, the first of the "broad issues" relates to the ambiguous nature of culture in causal models of accounting and control practices. The author means that, in extant accounting studies, culture usually acts as an independent variable, even if studies from outside the accounting and control literature conceive culture as a dependent variable. This is quite a straightforward issue, but the author then formulates the open question in rather indirect terms: "how can culture be captured as a construct that is being influenced by control?" (p. 5) A second "broad issue" relates to the distinction between control and culture, as management control typologies sometimes include culture as a type of control. The related "open question" is, "how can culture be captured as a transcendental concept in the context of control?" (p. 6) Three remaining questions point to similar and other ambiguities in the extant literature about the relationship between control and culture and lead to three other open questions. It remains somewhat unclear whether these questions aim to provide the reader with a roadmap to the analysis, or are in fact just illustrations of the logical and semantic complexities of the control and culture debate. In sum, therefore, the brief introduction of "challenges," "broad issues," and a "research question" leaves the reader somewhat confused as to the nature of the theoretical model that will be developed and validated.

The remainder of the introductory chapter is concerned with mapping out the objectives of the study, the outline of the empirical research design, and an overview of the chapter structure of the book. As regards the objectives, the author emphasizes the aim to contribute to theory as well as practice, pointing out "research gaps" in academic studies and the need for a "deeper knowledge" in practical situations. The research design proposes a qualitative field study that aims to result in an empirically founded framework for the relationship between control and culture. This is why the book is subtitled "from practice to theory."

The remainder of the book is clearly and logically organized into parts and chapters. Part I deals with the "groundwork" of the study and consists of the Chapters 2 and 3. Chapter 2 ("Basics") discusses the various extant definitions and connotations of the concepts of internal control and organizational culture. Since the study is placed in the context of failures of the financial industry (p. 1), it follows that much attention is paid to the effectiveness of internal control (p. 26). Here the author "broadly" (p. 27) defines internal control effectiveness by the extent of legal compliance, reliability of financial statements, and management's assurance that operational effectiveness and efficiency are achieved. It is not entirely clear how this definition supports the analysis. Indeed, while argued to be broad, the definition in fact narrows down internal control to *ex post* assessments via formal systems. While the author seems to suggest on the one hand that management control and internal control are essentially the same, as he refers to typical management control frameworks (e.g., Ouchi 1979; Merchant 1985; Simons 1995), on the other hand he seems to deemphasize the primary focus of management control on good decision making *ex ante*, as well as the fact that these frameworks all include organizational culture. Overall, this left me somewhat confused about the exact definition of internal control that will appear in the final framework. Why does the similarity of internal control and management control receive so much emphasis, after they have been demarcated sharply in Chapter 1 (p. 4)? Why, instead, is no attention paid to the obvious connection between internal control and accounting information systems? Can internal control indeed be seen as including management control, even if it excludes organizational culture? The author continues by arguing that internal control systems, even if "well designed" (p. 30), may not be immune to fraud and errors, and should be defined with a cost-benefit trade-off in mind. It does not become clear, however, how these two characteristics can or should be part of any foundational definition of internal control. The remainder of the second chapter discusses the concept of organizational culture. It leads the reader along various descriptions of organizational culture, resulting in the definition of culture as the ways in which groups of people adapt to the external environment and achieve internal integration (pp. 38–39). Although the definition is intended to exclude formal control arrangements, it does not do so explicitly.

Chapter 3 ("Literature Review") presents a well-structured overview of the conceptual and typological literature on internal control and management accounting and control. After a short section on academic research on internal control, it continues by evaluating the presence or absence of culture in the various internal control frameworks (i.e., COSO, CoCo, Turnbull, etc.). It also assesses the role of culture in management control typologies (e.g., Ouchi 1979; Merchant 1985; Simons 1995) and in a limited number of academic studies on the culture-control relationship. Overall, the findings illustrate the contention expressed earlier that culture is sometimes seen as a "part of" control, and sometimes as an external contingency that affects or interacts with control. The chapter closes with the (correct) conclusion that research on internal control and culture has not yet developed beyond its infancy. In response to this research gap, five "theses" are developed that provide another five guidelines to theory development about the culture-control relationship. An example is the first thesis that "[c]ause and effect between culture and control go in both directions" (p. 65). Taken by themselves, these five theses are a logical conclusion to the chapter. However, in combination with the earlier "research question," the two "challenges," and the five "open questions," a rather complex roadmap emerges that guides the analysis.

Part II, which consists of Chapter 4 ("Field Study"), presents interview feedback from 31 semi-structured interviews the author had with senior officials in 21 companies in Switzerland and in the U.S. (pp. 74–84). Interviews were conducted in two rounds and form the single source of empirical data. A first round of nine interviews in Switzerland aimed at obtaining a "practical understanding of internal control matters in companies" (p. 83). The second round of 22 interviews conducted in the U.S. focused on two questions. First, interviewees from various functions in management, finance, and auditing were asked to identify the principles and practices that they perceive as drivers for control effectiveness. The second question asked the interviewees to identify the roots of control failures. Following a Grounded Theory methodology for content analysis, the author describes the process through which interview transcripts were made, analyzed, grouped, and summarized. This results in the identification of five categories of control failures (related to "commitment," "competence," "communication," "complexity," and "change") and, within each of those categories, five drivers for control effectiveness. These latter drivers describe managerial practices that enhance internal control and mitigate the control problem. For example, practices that mitigate "commitment" failures include "lead by example," "deal with reality," and "ensure accountability." While this framework appears to be plausible, at first sight, the author seems to acknowledge its diversity and lack of clear dimensionality. Therefore, he proposes to interpret the framework as a supportive tool for classification of principles and practices, rather than as the outcome of strict classification. For example, he notes that "the drivers interrelate with each other and often influence several [control failure] areas" (p. 88). The chapter concludes by presenting an overview of illustrative interview quotations for each of the categories identified, which mention the management practices used to address these failures. Overall, this chapter provides a rich but relatively unstructured overview of internal control challenges and solutions from a selected set of companies and senior officials.

Part III aims to present "theoretical explanations" for the empirical findings from the earlier part. Chapter 5 ("Capturing Culture") sets the scene by presenting culture as a "black-box" that needs to be opened (p. 120). It postulates that we could conceive of culture both as a variable and as a metaphor, and proposes to investigate how culture mediates the relationship between management principles and practices and internal control effectiveness (pp. 120–122). This provides a setup for the analyses in the two subsequent chapters. These analyses distinguish between "the organizational level" (Chapter 6) and "the individual level" (Chapter 7). Chapter 6 relies on social system theory to present culture as an organizational phenomenon, which provides a buffer between the external environment and the internal environment. Via a series of graphical illustrations, the chapter introduces the concepts of "boundaries" between the organization's external environment and its culture, and between its culture and its internal environment. The concept of "transfer" between these three spheres is introduced to explain how behaviors and values are adopted and copied from one sphere to another. It concludes by suggesting that these boundaries may be "open" or "closed," thus enabling or hindering the transfer of behaviors and values between environments and culture. Chapter 7 reiterates this analysis at the individual level, loosely guided by the question of how people's individual interests and values affect the individual's inclination to engage in dysfunctional behavior. It stresses the importance of alignment between individual and organizational values and interests, which is an important, if not a rather undisputed claim.

Part IV ("Synthesis") combines the various steps in the theoretical and empirical analyses into an overall framework. In fact, a surprisingly simple "control-and-culture framework for effective control" is introduced and graphically illustrated (p. 163), which relies on three types of "drivers" that are defined as including "formal and informal control mechanisms, but ... also include other factors influencing behavior" (p. 161). The main thrust of this model is that managerial principles and practices provide drivers that may "open" or "close" the "boundaries" between individuals, the culture of the organization they work in, and the external environment in which the organization operates. Closing drivers are those that define how controls need to be performed and how organizational members should act (p. 162). They define acceptable behavior within the culture and provide consistency throughout the culture (p. 165). Opening drivers give those members some freedom to act and adapt (p. 162). They encourage interaction, debate, and accountability (p. 167). A third category of "reinforcing drivers" refers to those that support and communicate the opening and closing drivers that are present (p. 169). On first consideration, this threefold typology is elegant and plausible, as it seems solidly based on the author's account of practice. On second consideration, however, I became a bit concerned with the descriptive and predictive validity of these three kinds of drivers. Can all managerial practices and principles be so neatly ordered to be either of the "opening," "closing," or "reinforcing" type? Are these categories of mechanisms and other factors influencing behavior solid and precise enough to inform theory and practice? Is the difference between "opening," "closing," and "reinforcing" drivers the same at the organizational level as at the individual level, as Chapters 6 and 7 suggest? The remainder of the chapter seems to address these validity concerns by pointing out applications of the new framework. In particular, it gives some examples of how the framework may inform theory and practice. Regarding the former, it shows how the new framework compares to the Merchant and Simons typologies. This is an interesting exercise, even if both extant frameworks are not primarily theory-based or even research-based frameworks. Regarding practice, the author gives some examples of how "opening," "closing," or "reinforcing" drivers (should) reflect in managerial and auditor behavior. The chapter closes by a short review of how the five theses, which were formulated in Chapter 3, can be interpreted in the control-and-culture framework. The chapter furthermore sketches strengths and limitations of the research design. Chapter 9 ("Conclusion"), finally, provides a short overview of the study's questions, design, and outcomes, and concludes with an overview of further research opportunities.

Overall, this study provides a much-needed next step in academic research aimed at understanding the relationships between such complex phenomena as "internal control" and "organizational culture." The choices made in this study regarding theory and method, however, will provide quite a tough read to those expecting an unproblematic analysis in the tradition of contemporary management control studies. Neither contingency theory (dominating behavioral control studies) nor economic theory (dominating agency-based control studies) is explicitly addressed, such that the theoretical foundation of this study appears to be a bit more opaque than in extant research, and to be based on implicit rather than explicit theoretical considerations. Given that the study is empirically founded on only a limited number of observations, especially the author's first suggestion, which is to find generalization of the proposed claims in future studies, deserves support. Indeed, the study provides more than enough theory, empirics, and concepts to make it a starting point for such further enquiries, even if its relative complexity and lack of conceptual, theoretical, and empirical precision does not point to any clear, particular way forward.

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FRANK HARTMANN
Professor of Management Accounting and
Management Control
Erasmus University

EDITORIAL POLICY AND STYLE INFORMATION

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The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

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107 UNIVERSITY OF EVANSVILLE is seeking applicants for a tenure-track position in Accounting at the Assistant/Associate Professor level. The successful candidate will have the responsibility for teaching required and elective courses in the accounting area. Strong teaching interests in accounting with secondary interests in finance are preferred. In addition to teaching responsibilities, the position will require that the faculty member engage in scholarly activities leading to the publication of research in refereed journals. Faculty members are expected to participate in service activities to support the University's mission. An earned doctorate, preferably in business administration with a specialization in accounting from an AACSB-International accredited program is required. ABD applicants will be considered with the expectation that degree requirements will be completed prior to employment. A record of teaching and scholarly or creative activity would be required to merit appointment at the rank of Associate Professor. In addition, professional work experience and CPA licensure is highly desirable. Knowledge of global business applications is a plus. Contact information: Dr. James Schaefer at: js2@evansville.edu.

108 WESTERN NEW ENGLAND COLLEGE invites applications for a tenure-track Assistant/Associate Professor accounting position to begin August 2011. Candidates are expected to hold a doctorate in accounting or anticipate having completed all degree requirements by the starting date. The successful candidate will demonstrate a strong commitment to teaching in the undergraduate and graduate programs and to publishing scholarly research. An interest in teaching forensic accounting is desirable. Western New England College is a private, independent, coeducational institution founded in 1919 and located on an attractive 215-acre suburban campus in Springfield, Massachusetts within easy access of Boston and New York City. Send *vita*, teaching evaluations, research manuscript, and reference information to: Dr. Julie Siciliano, Dean, School of Business, Western New England College, 1215 Wilbraham Road, Springfield, MA 01119. Electronic submissions are encouraged and should be sent to: ajacque@wnec.edu. More information about the College is available at http://www.wnec.edu. Western New England College is an Equal Opportunity Employer.

109 THE UNIVERSITY OF IOWA, Department of Accounting, Tippie College of Business invites applicants for a tenure-track appointment(s) beginning fall 2011. We will consider candidates at all ranks. Required qualifications include a demonstrated interest in and capacity to do research publishable in top-tier accounting journals, a high level of teaching competence, and an earned doctorate (entry-level candidates can be near completion). We offer a competitive compensation package. Required qualifications include a demonstrated interest in and capacity to do research publishable in top-tier accounting journals, a high level of teaching competence, and an earned doctorate (entry-level candidates can be near completion). We will screen applicants at all ranks on an ongoing basis. To apply, please log onto our website, https://www.biz.uiowa.edu/recruiting/accounting, to upload your résumé, representative research output, evidence of teaching capabilities (if available), and to provide the names for at least three letters of recommendation. The University of Iowa is an Affirmative Action/Equal Opportunity Employer. Women, minorities and individuals with disabilities are encouraged to apply.

110 RUTGERS UNIVERSITY, Rutgers Business School-Newark and New Brunswick invites applications for a full-time tenure-track position(s) at the Assistant/Associate/Full Professor level in the department of Accounting and Information Systems. All ranks will be considered. Job Qualifications: A strong commitment to excellence in high-quality innovative research and teaching at the undergraduate and graduate levels is expected. A Ph.D. degree from an AACSB-accredited school in the field of accounting or a related discipline is required. However, candidates at the Assistant level who are close to completion of their degree will be considered. Candidates at the Associate or Professor level must have an outstanding research record commensurate with the level of appointment. Preference will be given to candidates who specialize in accounting information systems, auditing, or financial accounting. Teaching load and salary are competitive with research universities. Application Procedure: The review of applications will begin immediately and applications will be accepted until the position is filled. Apply electronically by sending your application via email to: Professor Miklos Vasarhelyi, Search Committee Chair at: miklosv@andromeda.rutgers.edu. Only electronic submissions will be accepted. Please attach the following documents with your electronic submission: application letter, *vita*, and job market paper. Finalists will be asked to forward official transcripts, three letters of reference, and other supportive materials as requested by the Search Committee. Rutgers University is an Affirmative Action/Equal Opportunity Employer. Women, members of minority groups, and persons with disabilities are encouraged to apply.

111 CALIFORNIA STATE UNIVERSITY, NORTHRIDGE, Department of Accounting and Information Systems is accepting applications for a tenure-track position at the Assistant Professor level beginning fall semester 2011. Responsibilities include teaching undergraduate and graduate courses, engaging in an ongoing program of scholarship, and assuming an active role in the planning and governance of the institution. For more information visit the college (http://www.csun.edu/busecon) and the department (http://www.csun.edu/acctis) websites. Qualifications: Doctoral degree in Accounting or a related field (including Education) from an accredited institution. Applicants with a J.D., a Master's in Accounting, a CPA, or CMA, as well as teaching experience and research agenda in accounting are encouraged to apply. Evidence of degree(s) required at time of hire. At time of appointment, the successful candidate, if not a U.S. citizen, must have authorization from the Bureau of Citizenship and Immigration Services to work in the U.S. Screening continues until position filled. Interested candidates should submit a *curriculum vitae*, three letters of recommendation with complete contact information, and a letter of application that highlights accomplishments and addresses the qualifications and responsibilities described in the formal position announcement. Address inquiries to: Paul.Lazarony@csun.edu. Send applications to: Search and Screen Committee, Department of Accounting and Information Systems, College of Business and Economics, California State University, Northridge, 18111 Nordhoff Street, Northridge, CA 91330-8372. CSUN is an Equal Opportunity/Affirmative Action Institution.

112 MERCER UNIVERSITY, Eugene W. Stetson School of Business and Economics (SSBE), an AACSB-International accredited institution, is seeking applicants for a tenure-track position in Accountancy at the Assistant or Associate Professor level, on our Macon campus to begin August 2011. This position offers the opportunity to balance teaching, research, and service to the University. We are searching for a person who has particular interests in Managerial and Cost Accounting, but will consider those with interests in other areas as well. A Ph.D. or D.B.A. with an accounting concentration from an accredited institution with demonstrated teaching excellence and an active research agenda is required. Professional certification and relevant work experience is desired. Salary is commensurate with experience. *The Princeton Review's* 2010 "Student Opinion Honors for Business Schools" ranks SSBE in the top 15 nationally in the areas of marketing and accounting. *The Princeton Review* also recognized Mercer's business programs among the nation's elite in its 2011 edition of "Best Business Schools." The guidebook has also ranked Mercer's Atlanta M.B.A. programs No. 3 in the nation in the category of "Greatest Opportunity for Women." For 21 consecutive years, Mercer University has been ranked among the top 15 regional universities in the South by *U.S. News & World Report.* The campus is located in historic Macon, GA in a traditional residential environment. Interested applicants must apply online at: http://www.mercerjobs.com. AA/EOE/ADA.

113 NORTHEASTERN UNIVERSITY is seeking applications for tenure-track positions effective September 2011 at the Assistant or Associate Professor rank. We seek candidates who have or expect to have a doctorate in accounting or a closely related business field before the start date of September 2011 and who are committed to excellence in research and teaching. A strong research program focused on top-tier accounting journals is desired and teaching experience in accounting is preferred. While the group needs faculty to teach in all areas, it has particular needs for faculty to teach all levels of financial and managerial accounting. The accounting group provides a supportive environment where collaboration in research and teaching is encouraged. The position offers competitive compensation and teaching responsibilities that enable a significant level of high-impact research. Rank and salary are dependent on qualifications. Summer funding, graduate assistants, and travel support are available. Accounting courses are offered at both the undergraduate and at the graduate levels (M.B.A., M.S./M.B.A., M.S.A., and M.S.Tax). Please submit your application online by visiting the College website at: http://www.cba.neu.edu/faculty/ and clicking on Faculty Positions. Include a cover letter, your current *cv*, and two samples of your scholarly work. Questions regarding this position may be directed to: Dr. Ganesh Krishnamoorthy at: g.krishnamoorthy@neu.edu. Northeastern University is an Affirmative Action/Equal Opportunity/Title IX Employer and welcomes applications from minorities, women, and disabled persons.

114 Fort Lewis College, School of Business Administration, Durango, Colorado invites applications for a tenure-track position in Accounting starting Fall 2011. The School seeks individuals who are energetic and innovative in the classroom. Requires Ph.D./D.B.A., proven teaching excellence, published scholarship, and commitment to college service. Professional certification (CPA/CMA) highly desirable. Teaching responsibilities include introductory financial, managerial, advanced financial, and either audit or governmental and not-for-profit. Send application letter, *vita*, and list of three references to: Rose Clay, School of Business Administration, 132 EBH, Fort Lewis College, 1000 Rim Dr., Durango, CO 81301; or Email: Clay_R@fortlewis.edu. The position will remain open until filled. Fort Lewis College does not discriminate on the basis of race, age, color, religion, national origin, gender, disability, sexual orientation, political beliefs, or veteran status. Accordingly, equal opportunity for employment, admission, and education shall be extended to all persons. The College shall promote equal opportunity, equal treatment, and affirmative action efforts to increase the diversity of students, faculty, and staff. People from under-represented groups are encouraged to apply. For details visit: http://www.fortlewis.edu/jobs.

115 CINCINNATI METROPOLITAN HOUSING AUTHORITY (CMHA) is actively recruiting for a Chief Financial Officer (CFO). The CFO will direct and oversee Finance, HR, and Procurement Staff while maintaining responsibility of financial operations for the Authority. CMHA currently has an operating budget in excess of $30 million per fiscal year, and numerous programs and special funds for a total annual budget in excess of over $130 million. The CFO will establish and maintain appropriate financial controls and accountabilities to ensure fiscal integrity and cost-efficient operations. The ideal candidate will have: Bachelor's degree in accounting, finance, or related discipline; ten years of accounting/finance management experience as CFO or similar position with organizational responsibility for accounting, budgeting and financial reporting; ability to function in a complex and changing affordable housing or regulatory industry; Generally Accepted Accounting Principles (GAAP) audited financial statement experience; working familiarity with financial applications on a computerized database; demonstrated ability to function in a complex and changing industry; demonstrated leadership and team-building abilities; professional communication skills; and demonstrated ability to present complex financial information. To be considered applicants must submit resumes, applications, and salary requirements. CMHA offers a professional work environment along with a competitive salary and excellent benefits. For a full description of this and other CMHA job opportunities, you may visit our website at: http://www.cintimha.com. Please mail, fax, or email your resume and salary requirements to: Cincinnati Metropolitan Housing Authority, Human Resources Department, 16 West Central Parkway, Cincinnati, OH 45202; Fax: (513) 977-5606; Email: hr@cintimha.com.

116 MCGILL UNIVERSITY, Desautels Faculty of Management invites applications for tenure-track positions in all areas of accounting at any level. Candidates must have a Ph.D. or be in the final stages of their dissertation. Preferred candidates have a proven record or demonstrate high potential for quality research and teaching. Located in downtown Montreal, McGill University is one of Canada's leading research universities and is well known for its academic excellence. Applicants should send their *curriculum vitae*, three letters of recommendation, and recent publications/working papers to: Professor Steve Fortin, Area Coordinator-Accounting, Desautels Faculty of Management, McGill University, 1001 Sherbrooke St., West Montreal, Quebec Canada, H3A 1G5. All qualified applicants are encouraged to apply; however, Canadians and permanent residents will be given priority. McGill University is committed to equity in employment and diversity. It welcomes applications from indigenous peoples, visible minorities, ethnic minorities, persons with disabilities, women, persons of minority sexual orientations and gender identities, and others who may contribute to further diversification.

117 UNIVERSITY OF ILLINOIS AT CHICAGO, College of Business Administration seeks applicants for tenure-track faculty position(s) beginning August 2011. The position will encompass not only research, but also undergraduate and graduate teaching and outreach to the Chicago business community. A demonstrated research record is expected, Ph.D. in Accounting by August 16, 2011. For consideration, complete the online application at: https://jobs.uic.edu/ attach a letter of interest, *curriculum vitae*, representative publications, and references; only completed applications with all documents attached will be considered. UIC is AA/EOE.

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